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## WILLIAM HARVEY AND THE PHILOSOPHY OF SCIENCE

By J. A. PASSMORE

The discovery that the blood circulates in the body is, on the face of it, a bare empirical fact, of no interest to the philosopher *qua* philosopher. Yet when, in the Epistle Dedicatory to his *Elements of Philosophy*, Hobbes names his predecessors, William Harvey is the only Englishman in a list which includes Kepler, Galileo, Gassendi and Mersenne. Similarly Descartes, in his *Discourse on Method*, devotes to Harvey a degree of attention which, to modern eyes, is entirely disproportionate; the honour of being mentioned in the *Discourse* Harvey shares only with Galileo.<sup>1</sup> That both Hobbes and Descartes should pay tribute to Galileo is, it would generally be conceded, only fitting; for Galileo is one of those few scientists who have revolutionised our attitude to the world around us. But why Harvey? What does it matter, except to physiologists and surgeons, whether the blood circulates or stands still?<sup>2</sup>

The importance of a scientific discovery to philosophy, however, has much more to do with the state of thought at the time of its enunciation than with its content. The synthesis of urea, the discovery of reflex action—these two were in their scientific content no more important than many another scientific discovery, but their philosophical impact was of a special order.<sup>3</sup> Descartes said of Harvey that he “broke the ice”; it is this “ice-breaking” character of Harvey’s work which explains why

<sup>1</sup> Note also Henry More’s comment, referring to a work of Cudworth’s, that it is “of as much price and worth in theology, as either the circulation of the blood in physic, or the motion of the earth in natural philosophy” (Preface to *The Grand Mystery of Godliness*, 1660).

<sup>2</sup> Thus in their edition of Descartes’ philosophical writings (Edinburgh, 1954) Anscombe and Geach omit this whole section of the *Discourse* as “a long passage on physiology, in particular the circulation of the blood, which is now of merely historical interest” (note to p. 41).

<sup>3</sup> Cf. ch. 2 in J. A. Passmore: *A Hundred Years of Philosophy*, London, 1957.

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it was immediately seized upon by Hobbes and Descartes—Descartes accepted the circulation of the blood at first hearing, before he had read *De Motu Cordis*<sup>4</sup>—in sharp contrast to its sceptical, or even hostile reception at the hands of anatomists.

For Harvey broke through a barrier; he proclaimed a unity where there had seemed to be an absolute gulf: between the animal organism and inanimate mechanical systems. Copernicus and Galileo had shown that continuous motion is not the prerogative of heavenly, as distinct from terrestrial, movements; they had united the heavens and the earth in a single mechanical system. But the place in that system of the human being—and, more generally, of the animal organism—had still to be determined.

Harvey detected circular movement in the body: that fact was at once seized upon by ardent seventeenth-century generalisers—most notably, but not solely, by Hobbes and Descartes—as a hard core to, a point of empirical reference in, their speculations. The principles of mechanics, it was now said, apply to the behaviour of all material objects, including the inner workings of the living organism. This daring speculation, it will at once be obvious, in no sense followed from Harvey's discovery; any more than it followed from the synthesis of urea that the structure of all organic substances is physico-chemical. But Harvey destroyed, in a single blow, the *a priori* supposition that it is in principle impossible to describe organic processes in mechanical terms. Thus he opened the way to a general view of the nature of things.

That general view, however, still leaves room for diverse possibilities; Descartes and Hobbes incorporated Harvey's discoveries in quite different philosophical systems. For Hobbes, the extension of mechanical explanation to the behaviour of living organisms was but a step on the way to an explanation, in the same terms, of the processes of the human mind and of human society. All the same, this first step was a crucial one: once grant that the behaviour of the living organism falls within the scope of the laws of mechanics, and it seems natural to conclude that this is as true of the conscious life as of any other form of behaviour of the living organism. The onus of proof shifts—that is Harvey's importance. In the past, mechanists had been confronted by an absolute barrier at the living organism;

<sup>4</sup> Cf. E. Gilson: "Descartes, Harvey et la scolastique" in *Etudes sur le rôle de la pensée médiévale dans la formation du système cartésien*, Paris, 1951. See also Gilson's edition of, and commentary on, Descartes' *Discours de la Méthode*, 1947, and H. P. Bayon's articles on Harvey in *Annals of Science*, Vols. III and IV, 1938.



the writ of mechanism extended only to the inanimate. After Harvey, on the contrary, those who still maintained that mental processes are non-mechanical were making of the human mind a single exception in an otherwise mechanical world.<sup>5</sup>

Descartes, of course, was in this position. He hoped, that is, to lay down a boundary at a new point, no longer between the animate and inanimate, but between an unextended rational soul and an extended mechanical world. The soul, alone amongst finite entities, stands outside the reach of mechanics. Many of Descartes' admirers, particularly in England, regarded his physiology with dismay. He had given away too much, they felt; once surrender the animal organism to the mechanist, and the soul will not for long be impregnable.<sup>6</sup> In the end, too, they were right; from beast-machine it was an easy step to man-machine. Descartes, not Hobbes, is the direct ancestor of modern medical materialism.

Unlike Hobbes, Descartes had a special interest in Harvey, of the sort we should now call "scientific" rather than philosophical. Hobbes, of course, was well aware of the nature and extent of Harvey's achievement in this respect—Harvey is, Hobbes wrote in his Dedicatory Epistle, "the only man I know that, conquering envy, hath established a new doctrine in his lifetime"—but his attitude is that of a respectful outsider, whereas Descartes was a leading participant in the physiological revolution. Hobbes is content to draw attention to the general importance of Harvey's work; Descartes examined it in detail. In important respects, indeed, he rejected Harvey's conclusions—although never the all-important circulation of the blood—and reverted, as Gilson has shown in detail, to certain of the teachings of his medieval predecessors.

At first sight, the differences between Harvey and Descartes are merely technical, more than a little tedious, except to those who take a detailed interest in the history of physiology. Descartes, it might be supposed, made a scientific mistake: he wrongly supposed that the heart performed its function in virtue of its containing a peculiar source of heat. No more need be said. In fact, however, the ground of dispute lies deeper; and

<sup>5</sup> Of course, once more, there was nothing in Harvey's work to prove that they were wrong. The fact remains that the whole question took on quite a new aspect. Compare J. J. C. Smart: "Plausible Reasoning in Philosophy", *Mind*, 1957, 261, 75-8.

<sup>6</sup> Compare Cudworth's judgement of Descartes' writings in his *True Intellectual System of the Universe*: they have, he wrote, "an undiscerned tang of the mechanically-atheistic humour hanging around them". (See, on Cudworth's relation to Descartes, J. A. Passmore's *Ralph Cudworth*, Cambridge, 1951). See also Huxley's essay "On the Hypothesis that Animals are Automata" in *Method and Results*, London, 1893.

illustrates a second respect in which Harvey's work is important for philosophy.

Harvey, in Sir Henry Dale's words, "created and displayed for all time the method by which such (biological) discovery may be established and made secure."<sup>7</sup> The real question is whether, in principle at least, biology has a method of its own, as Harvey's work suggests, or whether it is a branch of geometry; and whether, further, unless it is geometrical in structure, it can have the slightest claim to be regarded as a science. Descartes and Harvey agreed that the body is a mechanical system; but Descartes argued, and Harvey denied, that the biologist, if he has any scientific pretensions, must make use only of concepts which are in principle derivable from the general concepts of mechanics. If, as Descartes thought, the heart is merely a container within which the blood is heated to the point of expansion, then whatever is of scientific interest in its operation is a simple exemplification of broad mechanical principles. If, however, as Harvey tried to show, the heart is a contractile muscle, then it has peculiar and irreducible properties, not characteristic, even, of physiological process in general, let alone of inanimate nature. Harvey had suggested that the circulation of the blood rests on a "brute fact"—the fact that the heart is a contractile muscle—and this is equivalent, Descartes argued, to abandoning science altogether. Harvey, he alleges, has offered no genuine explanation of the circulation of blood, but only a pseudo-explanation, an explanation by "faculties".

This allegation, one might be inclined to think, rests on a simple misunderstanding. On several occasions, in expounding his theory of the circulation, Harvey made use of the phrase "*facultas pulsifica*" to describe the mode of operation of the heart. It is clear enough to us what he meant: no more than this, that the heart is in fact a contractile muscle. But to Descartes, as to all the seventeenth-century revolutionaries, the word "*facultas*" suggested all that was worst in scholasticism—the pseudo-science of "*occult faculties*". In *La description du corps humain* (written in 1648-9) Descartes wrote of Harvey as follows: "If we suppose that the heart moves in the manner in which Harvey described it we shall have to imagine some faculty which causes this movement, the nature of which is much more difficult to conceive than everything he claims to explain by it."<sup>8</sup> Here surely, one is tempted to conclude, Descartes has simply been misled by Harvey's "*facultas pulsifica*".

<sup>7</sup> "Harveian Oration", *The Lancet*, 1935, 229, p. 927.

<sup>8</sup> *Works*, ed. Adam and Tannery, Vol. XI, p. 243.



Yet, it should be noted, Descartes does not say, simply, that Harvey *in fact* refers to faculties; he says he *must* do so; that on his view of the heart's action Harvey is forced into faculty explanations by the very logic of his case. To understand Descartes' arguments one must appreciate the rôle of brute facts in his philosophy. For Descartes the mathematical-physicist, as for many of his successors in that same tradition, the position is perfectly clear; brute facts are at best a stimulus to thought, a prop to the defective powers of the human intellect. Experiment and observation, that is, could in principle be wholly dispensed with; for all the propositions of mathematics and physics are deducible from first principles, which are innate in the intellect's own operations.

But there are *two* Descartes—Descartes the mathematical-physicist and Descartes the physiologist.<sup>9</sup> As a physiologist, Descartes took a less exalted view of the intellect's unaided powers. As Roth points out, the (little read) sixth book of the *Discourse on Method* is a confession of failure—a confession provoked by the breakdown of the Cartesian physico-mathematical method when it is confronted by the multifarious diversity of biology. "The potentialities of Nature are so extensive and diverse", Descartes writes, "and my principles so simple and general, that I have hardly observed any particular effect that I could not recognise as being deducible from the principles in a variety of ways; and my main problem, as a rule, is to find out in which of these ways the effect does depend on them. To advance that object, I know of no other expedient than to look out once more for experiments that would give different results, according as one or the other explanation is right."<sup>10</sup>

Experiment, then, is necessary in biology;<sup>11</sup> but all the same its rôle is a limited one. Every biological fact is deducible, Descartes is still saying, from first principles; the difficulty lies in the fact that there is an embarrassment of possible ways of tracing it back to those principles. Thus, to take the point at issue, it could be the case *a priori* that the liver, not the heart, is the area within which the blood is so heated as to be kept in circulation; only by experiment, then, can we articulate the chain connecting the facts which confront us—e.g. the circulation of the blood—with the general principles of mechanics. The fact remains that, for Descartes, there is no science before the

<sup>9</sup> Compare, on this point, Leon Roth: *Descartes' Discourse on Method*, Oxford, 1937.

<sup>10</sup> ed. Gilson, Paris, 1947, pp. 64-5.

<sup>11</sup> See also the Author's letter which Descartes prefixed to *The Principles of Philosophy*, 1647.

chain is laid bare i.e. there is nothing scientific in the mere discovery that the heart plays a part in the circulation of the blood.

Thus Descartes does not, in principle, abandon the claim that science is mathematical demonstration, even when he grants that only with the help of experiment can we select between various demonstrations, each of which is possible *a priori*. Introducing in the fifth part of the *Discourse* the circulation of the blood, he addresses his potential critics in the following terms: "In order that those who are not acquainted with the strength of mathematical demonstrations, and are not used to distinguishing true from plausible reasons, should not venture to deny what I have said without examining it, I wish to advise them that this movement which I have just described follows as necessarily from the mere disposition of the organs which the eye can see in the heart, and the heat which we feel with the fingers, and the nature of the blood which one can learn by experiment, as does that of a clock from the form, position and shape of its counter-weights and its wheels."<sup>12</sup>

Descartes obviously believes, then, that he has succeeded in providing a mathematical demonstration of the heart's movement and this is equivalent, on his view, to a demonstration that its movement follows from the general concepts of mechanics. Expansion under the influence of heat is a quite general mechanical mode of behaviour. Descartes is perfectly willing to suppose that there is in the heart "a fire without flame", that processes of expansion go on which are not directly observable, because this is in his eyes *the right kind of explanation* i.e. one which has general application to a wider range of mechanical processes. But he is not prepared to admit that the movement of the blood depends upon the contractile properties of the heart, because this would be a *special* explanation; one which is not applicable, even, to other physiological processes, let alone to mechanical processes in general. Thus he identifies, we might say, an appeal to "brute fact"—the fact that the heart operates in such-and-such a special way—and an appeal to occult qualities; he sees in Harvey's theory a species of obscurantism, because Harvey appeals to something the existence of which has just to be accepted as a fact.

"What else", he asks, writing to Plemp, "could be the cause of very great and sudden changes in the body except boiling and fermentation?"<sup>13</sup> Boiling and fermentation are "intelligible"

<sup>12</sup> ed. Gilson, p. 50.

<sup>13</sup> *Works*, Vol. I, p. 531.



and general causes; nothing less will satisfy Descartes' ambitions. His criticism of Harvey, then, is of exactly the same form as his criticism of Galileo; as he wrote to Mersenne, "Without having considered the first causes of Nature, he (Galileo) has only looked for certain particular effects, and upon this has built without foundations".

Another objection Descartes raises is of a rather different kind: Harvey, he argues, has left a great deal unexplained. In particular, he cannot explain why the blood changes colour as it passes through the heart. Thus, Descartes argues, he is once again forced back upon hidden "faculties". Not only must he invoke faculties to explain the pulsating movement of the heart; he has then to introduce still further faculties "that change the qualities of the blood, while it is in the heart".

Against this onslaught what can Harvey reply? In a sense, only something which corresponds to Galileo's "*e pur si muove*"—"but still, the heart is a contractile muscle". This reply, however, involves, and leads Harvey into, the defence of a particular philosophy of science—a philosophy of science characteristic, we might say, of medical men and biologists.<sup>14</sup>

The main source for this philosophy of science is Harvey's "Second Disquisition to John Riolan" (1669), in which he explicitly replies to Descartes' anatomical observations, but also, it would seem—although in this case he makes no explicit reference to Descartes—challenges his philosophical presumptions. It is important to observe that he begins his Second Disquisition with a vigorous attack upon the medieval theory of "animal spirits"; he has obviously moved much further from medieval modes of explanation than Descartes ever did. "Persons of limited information", he says, "when they are at a loss to assign a cause to anything, very commonly reply that it is done by the spirits; and so they bring the spirits into play upon all occasions; even as indifferent poets are always thrusting the gods upon the stage as a means of unravelling the plot and bringing about a catastrophe."<sup>15</sup> Or a little later: "Those who advocate incorporeal spirits have no ground of experience to stand upon; their 'spirits' indeed are synonymous with powers or faculties." This is the man whom Descartes accuses of being a faculty-monger! Obviously, Harvey is writing very much in the manner of a modern positivist; this is the attitude of mind Descartes really

<sup>14</sup> It is no accident that Sextus Empiricus in the ancient world, and Locke in the modern world, were trained as doctors.

<sup>15</sup> *Works*, ed. R. Willis, Sydenham edition, p. 116.

had to meet, but did not understand. Like many another philosopher before and after him, he sought to assimilate the new to the old enemy: if Harvey could only be branded a "faculty" scientist, a dealer in "occult qualities", Descartes knew how to dismiss him with a phrase.

Now Descartes' argument really amounts to this: Harvey gives no explanation of the heart's contractility; to offer no explanation is tantamount to saying that the heart's contractility explains itself; and this in turn means that Harvey is really asserting that it contracts because it has the faculty of contracting. Not to explain, then, is logically equivalent to offering a faculty-explanation.

Harvey's reply is uncompromising: "I own that I am of the opinion that our first duty is to inquire whether the thing be or not, before asking wherefore it is."<sup>16</sup> This is the method to which Descartes objected in Galileo; the method of seeing what the "effects" are—i.e. what actually happens—and describing these "effects" quite independently of their relation to "first causes". For Descartes, science is demonstration; for Harvey, on the contrary, demonstration is a second-best method: the best thing to do is to *look and see*.

What the astronomer actually perceives, Harvey is willing to admit, is only a clue to what really happens. The situation in biology is altogether different: the biologist can observe directly what happens, with his own senses. In biology, therefore, where the typical method is dissection, "the example of astronomy", writes Harvey, "is by no means to be followed, in which from mere appearances or phenomena that which is in fact, and the reason wherefore it is so, are investigated".<sup>17</sup> The astronomer cannot directly observe an eclipse, Harvey argues, because he cannot take up his situation beyond the moon to see what happens; thus he is obliged to reason from mere appearances to their cause. The biologist, on the other hand, can dissect, i.e. can look for himself and see what happens. In this respect, then, biology is superior to such forms of inquiry as astronomy, for "no more certain demonstration or means of gaining facts can be adduced than examination by the senses, than ocular experiment".

Here, then, is a confident empiricism at the opposite pole from Descartes' depreciation of sensory observation. And we can see how the controversy between Harvey and Descartes, like so many subsequent controversies about "the nature of scientific

<sup>16</sup> *Works*, p. 122.

<sup>17</sup> *Works*, p. 124.



inquiry", is an attempt, on both sides, to maintain the status of their own particular form of inquiry, by arguing that its procedures form a paradigm for "science".

Thus just as Descartes argued that the biologist, like the geometer, must demonstrate, but that biology, because its demonstrations must be accompanied by experiment, is in some measure inferior to physics, so Harvey, on the other side, argues that even geometry rests upon sensory observation, but is inferior to biology in that it passes beyond what can be sensorily observed. "It is the business of geometry", he says, "from things sensible [i.e. geometrical figures] to make rational demonstration of things that are not sensible";<sup>18</sup> and from this he concludes—in the sharpest possible opposition to Descartes—that unless we can begin by trusting our senses absolutely, not even geometry could have a solid foundation. "I have not endeavoured", he sums the matter up, "from causes and probable principles to demonstrate my propositions, but, as of higher authority, to establish them by appeals to sense and experiment, after the manner of anatomists."<sup>19</sup>

Thus the dispute between Descartes and Harvey about the mode of circulation of the blood is a notable example of a head-on collision between two opposing philosophies of science. Harvey has seen the heart operating as a contractile muscle—most notably through the transparent body of a shrimp; that this movement could not be deduced from the elementary ideas of mechanics does not perturb him in the least—it is enough for him that it is a mechanical motion in the sense that *the heart moves in accordance with mechanical laws*. In the language of a later date, Harvey is not, and Descartes is, a "reductionist". Nor does it perturb Harvey that the same sort of explanation could not be offered of other physiological processes. Descartes, so far as Harvey is concerned, might be right in supposing that in other physiological processes heat is the agent; he is prepared to look and find out, with no *a priori* expectation that all physiological processes will be of a single pattern.

"There are persons", writes Harvey, "who will not be content to take up with a new system, unless it explains everything, as in astronomy."<sup>20</sup> This is his answer to Descartes' claim that an absence of explanation is, in principle, equivalent to a faculty explanation. Descartes was right in saying that

<sup>18</sup> *Works*, p. 137.

<sup>19</sup> *Works*, p. 138.

<sup>20</sup> *Works*, p. 123.

Harvey could not explain why the blood changed colour in the heart; no one could explain this until after Lavoisier's work on oxygen. But Harvey was prepared to leave a question-mark; he did not say that the blood was changed by an "infusion of spirits" or by a "rubific faculty"—he said that he did not know *how* it was changed—and thus he left the way open for Lavoisier's explanation. He knew what he knew; and was not going to be argued out of it, merely on the ground that there were other things he did not know. This again was an attitude of mind quite unintelligible to Descartes—but it is the attitude characteristic of empiricists.

Yet is one simply to say that Descartes was wrong? Of course, he was wrong about the movement of the heart; we would all agree about that. But, if we look at the matter historically, was his attitude to Harvey without justification? This is a much harder question to answer. Science needs for its development both its Descartes and its Harvey: the scientist who clings tenaciously to the fact he has observed, in the teeth of established theories, and the scientist who boldly attempts to formulate generalisations of the widest possible sort, in the teeth of "established facts". Neither the Harveys nor the Descartes of this world are always right; facts are not always what they at first seem to be, but if they are, no theory can explain them away. Descartes was wrong, I should argue, in supposing that there are "ultimate explanations"—for any explanation is itself a "brute fact"; nevertheless the search for such explanations has been a factor of crucial importance in the history of science. Harvey was right in standing by what he had observed;<sup>21</sup> but his positivism can easily be converted, as Descartes may have intuitively apprehended, into an obscurantist opposition to that speculative spirit which is the life of science.

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<sup>21</sup> It is interesting to notice, however, that many of Harvey's observations were completely ignored by his successors, *just because they did not form part of any theory*. Compare, for example, his remarks on the connection between bottled-up aggressiveness and heart disorder (*Works*, p. 127-8).



## HAS COLLINGWOOD BEEN UNFORTUNATE IN HIS CRITICS?

By G. BUCHDAHL

I wish to question certain views maintained by Mr. Alan Donagan, in his article "The Verification of Historical Theses"<sup>1</sup> respecting what he calls "the received interpretation" of Collingwood's theory of history. Collingwood, in his *Idea of History*<sup>2</sup> asks: What must an historian do in order to have historical knowledge (IH,282)? And his answer is: he "must re-enact the past in his own mind" (ibid.). Mr. Donagan holds that "Collingwood has been unfortunate in his critics" (D,207),<sup>3</sup> that his answer has been misunderstood. Instead of seeing it for what it is, namely a "philosophical" answer to a "conceptual" problem (D,203), involving an exploration of the "grammar" of 'think a thought' (D,204), his critics have wrongly taken Collingwood as making a methodological recommendation. I wish to show that Mr. Donagan may well be right in thinking that a very neat and satisfying situation would result from making this distinction between "philosophical" and "methodological" and coming down on one side of it, but that this isn't Collingwood. Secondly, it seems to me that if it were, although we should still be left with a large number of stimulating remarks concerning possible attitudes by historians towards their everyday work, there would be very little left that is philosophically exciting in Collingwood. Forcing his own interpretation on Collingwood, Mr. Donagan seems to me gravely to misunderstand what one might call the *tone* of a philosophical work that has metaphysical bearing. For in truth, what makes Collingwood interesting is surely that he uneasily hovers between both, using the phrase "re-thinking the past" as a piece of methodology *and* as expressing something with an epistemological or metaphysical function.

To start with, there are (as Mr. Donagan admits) a number of passages which very definitely seem to imply that re-enactment is something the historian is recommended to practise. In order to "reconstruct the history of a political struggle . . . the historian

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<sup>1</sup> Phil. Quart., 6:193-208, 1956.

<sup>2</sup> Oxford, 1946, hereafter referred to as "IH".

<sup>3</sup> Page references thus marked "D" refer to Mr. Donagan's article.

. . . must grasp their (the historical actors') political ideas" (IH,115), the "thoughts behind the events" (IH,118), thoughts that are the "inside of events" (ibid.). "His main task is to think himself into this action, to discern the thought of its agent" (IH,213). And "how does the historian discern the thoughts which he is trying to discover? There is only one way in which it can be done: by re-thinking them in his own mind" (IH,215). And to clinch it all, we have the celebrated passage to which Mr. Donagan himself refers as quite naturally supporting the contention that these remarks have a methodological function, whatever else they may have as well. I quote from Mr. Donagan's summary (D,202; cf. IH,283):

"Suppose that an historian studying the Theodosian code has before him an imperial edict: merely reading the words . . . does not amount to knowing their historical significance. In order to do that he must envisage the situation with which the emperor was trying to deal . . . as that emperor envisaged it. Then he must see for himself . . . how such a situation might be dealt with . . . the possible alternatives, and the reasons for choosing one rather than another; and thus he must go through the process which the emperor went through in deciding on this particular course. Thus he is re-enacting in his own mind the experience of the emperor; and only in so far as he does this has he any historical knowledge . . . of the meaning of the edict."

Does not this passage make sense only upon the assumption that Collingwood is here telling us what an historian must *do* if he is to be described as re-thinking the past? Not quite. For in addition to the methodological interpretation, which makes Collingwood say, "this is (the most important part of) what you must do when you are doing history", there is also the logical, or philosophical or metaphysical interpretation, "this is what you must have done (i.e. re-thinking Theodosius' thought) whatever procedural operation you may actually have gone in for, if you will be said to have obtained historical knowledge".<sup>4</sup> Now, surely *both* interpretations are legitimate?

Let us look at the difference between them. Consider the process-account. It looks as though what was essentially involved was simply the sort of reconstruction of which Collingwood has elsewhere given a very careful description, e.g. interpolation between the facts that function as "evidence", where what is to count as evidence is "what the historian can use as evidence" (IH,246-7). The success of such a construction ultimately will

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<sup>4</sup> Below it will appear that this formulation is itself double-edged.



depend on the know-how of the historian (IH, 238, 247). But there is also the psychological aspect. The historian's interpolative activity would run completely in the dark unless it were based on some sort of sympathetic understanding. And this is only possible (according to Collingwood) if what he pulls together are thoughts. And this means, not only that the material collected must consist of thoughts; but also that the historian must attempt to envisage these thoughts as the actor (e.g. Theodosius) lived them.

The following very colourful passage also supports such a view pretty powerfully:

"When I understand what Nelson meant by saying, 'In honour I won them, in honour I will die with them,' what I am doing is to think myself into the position of being all covered with decorations and exposed at short range to the musketeers in the enemy's tops, and being advised to make myself a less conspicuous target. I ask myself the question, shall I change my coat? and reply in those words. Understanding the words means thinking for myself what Nelson thought when he spoke them: that this is not a time to take off my ornaments of honour for the sake of saving my life. Unless I were capable—perhaps only transiently—of thinking that for myself, Nelson's words would remain meaningless to me: I could only weave a net of verbiage round them like a psychologist, and talk about masochism and guilt-sense, or introversion and extraversion, or some such foolery . . ."<sup>5</sup>

Be it remarked that this is only one of a possible number of characterisations; thus one may have "sympathetic understanding" of dogs, not only men. If Collingwood fastens on this particular aspect of historical work, he has a reason for it; a reason that connects with the point stressed by the "philosophical interpretation". To this I shall return presently.

And here I must plead guilty: in my article which Mr. Donagan discusses<sup>6</sup> I twice say that Collingwood nowhere tells us how re-enacting is to be done (B, III, 113). I was quite wrong. For has he not told us perfectly clearly, for instance in the Theodosius passage and the one just cited from the *Autobiography*, and in all his long discussions of the nature of historical construction and reconstruction? How could I have been so misled? However, Mr. Donagan's complaint is not that

<sup>5</sup> R. G. Collingwood, *An Autobiography*, Pelican, 1944, p. 77; hereafter referred to as A.

<sup>6</sup> "Logic and History: An Assessment of R. G. Collingwood's *Idea of History*," Aust. J. Phil., 26:94-113, 1948; page references to this article will be prefixed by B.

I did not find anywhere in Collingwood an explanation of what the historian must do in order to be described as re-enacting the past. On the contrary, he complains that I had demanded an explanation at all! And he does this because he believes that the only interesting question Collingwood can possibly be discussing is whether the historian, when he has "re-enacted" in accordance with the code of evidence, can be *correctly described* as having re-enacted, i.e. re-enacted in principle.

To sum up this part of the argument: Collingwood had asked what makes historical knowledge possible, and the reply suggests at least very strongly that he sometimes gives us a methodological account of the process which he spontaneously characterises as "re-enacting the past".

"But suppose the past lives on in the present; suppose, though incapsulated in it, and at first sight hidden beneath the present's contradictory and more prominent features, it is still alive and active; then the historian may very well be related to the non-historian as the trained woodsman is to the ignorant traveller. 'Nothing here but trees and grass', thinks the traveller, and marches on. 'Look', says the woodsman, 'there is a tiger in that grass.' The historian's business is to reveal the less obvious features hidden from a careless eye in the present situation. What history can bring to moral and political life is a trained eye for the situation in which one has to act.

"I expressed this new conception of history in the phrase: 'all history is the history of thought'. You are thinking historically, I meant, when you say about anything, 'I see what the person who made this (wrote this, used this, designed this, etc.) was thinking'."

Unless you try to do this, he says, you are not starting to do history, you are not obtaining any historical knowledge. Why should Mr. Donagan have been so certain that this was a "philosophical" and not a "methodological" remark? One reason is, of course, that he thinks that these are the only genuine alternatives, that anyone who treats passages such as A69 and A75 as a piece of writing about method, must *ipso facto* believe historical thinking to be *intuitive*. I don't think this is so. At A69, the historian is to help us see the past that lives on in the present, the "tiger in the grass". You may say: Surely he can only do this by the usual methods. You are quite right. But what is included in that term? Just the idea that we *don't* intuit?

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<sup>1</sup> A69 and A75.



The main trouble, however, is that Mr. Donagan thinks that one *must* interpret Collingwood's mode of talking as follows: Unless you re-enact the past *in the proper sense of that term*, historical knowledge is impossible; where *this* entails yet another question: Is the "proper sense" of that term defensible? A question that leads to an investigation of its logical grammar (D,204), an investigation called forth by qualms as to whether what has been characterised as "re-enacting" within the *procedural* account can "really" be called so, can, philosophically speaking, "really exist". (Cp. Collingwood: "I am considering how history as the knowledge of past thoughts . . . is possible" (IH,288).)

Now to *this* question the answer of course is not a procedural one, e.g. the Theodosius account. However, what sort of question is it, really? Someone asks: Can one re-enact the past? The answer might be: Don't be silly, of course you can. I have just given you a long account (e.g. the Theodosius or the Nelson story); *that* shows you how to do it. My interlocutor is not satisfied with this, and persists: But can one *really* re-enact the past? This is like the question in the logic of the natural sciences: can one discover physical laws? The obvious answer is: Surely we have been doing this sort of thing for 300 years? But what the questioner wanted, of course, was a *justification*. Such justifications used to be given in terms of duplicating para-accounts of perfectly natural facts, e.g. that nature was simple or uniform. Such a phrase, when descriptive, turns us back on to the laws we had always been *said* to have discovered; however, since it is intended here with a prescriptive, a metaphysical or justificatory use, the sense of "uniform" or "simple" must remain systematically or metaphysically open. It is a feature of the justification accounts that they *either* involve duplications of the descriptive or procedural accounts, *or* that they involve a description of certain sorts of para-processes, *or* that they end in a bare *yes!* or *no!*

In the same way, re-thinking the past is metaphysically ambivalent: it has ostensibly two grammars, a genuine one, and one not so genuine, more abstract. On the one side, it is whatever we have described it to be, e.g. the Theodosius account. On the other, we may attempt descriptions of certain sorts of models in terms of which to describe the "process". It is in these terms that Collingwood tells us that when I think about the "past activity of thought", I must revive it in my own mind, for the act of thinking can be studied only as an act. But what is so revived is not a mere echo of the old activity, another of

the same kind; it is that same activity "taken up again and re-enacted . . ." (IH,293). "Activity", Collingwood admits, is a bit of a misnomer. Nevertheless, if not this, then some other term will have to do. It is not too much to say that in a way Collingwood (though in abstract terms) simply reduplicates his earlier procedural account, and indeed, what else could he, could anyone, do? There *are* surely not *two* grammars of a term, one methodological and one logical?

The proposition "historical knowledge is possible on condition that the historian re-enact past thought (in the proper sense of that term)" on either interpretation, therefore, leads us to the procedural account, although ostensibly facing in two directions, viz. as a piece of methodological advice and as something meant to answer the request for metaphysical security, i.e. for a justification of the possibility of historical knowledge. There is, however, as yet a special reason which requires that both these aspects be considered together in order to appreciate properly the character of Collingwood's thought. This reason is the peculiar interpretation that Collingwood gives of, and the peculiar demand he makes on, anything to be called "scientific historical knowledge". In order to realise this, let us remind ourselves of the whole purpose of the re-enactment formula, and of the function it plays in Collingwood's work. Collingwood's method, one may say, makes a virtue of a supposed deficiency. The facts of history lie in the past; they are not open to perception, for they are no longer with us. Nor can we hope to proceed here by the probabilistic methods of the natural sciences, where the "bare facts" are made secure by relating them through laws (IH,214). Unfortunately, for the historian there are no such "bare facts", *subsequently* to be related. For to be in the possession of facts in this case, is to be in the possession of something that is *ipso facto* related to the interlocking network of neighbouring facts. To discover the facts is already to understand it, in one of Collingwood's felicitous phrases. Nevertheless, and surprisingly, Collingwood claims for historical argument that unlike the natural sciences it is capable of being "proved . . . as conclusively as a demonstration in mathematics" (IH,262). The "inference of the kind used in scientific history yields compulsion [and not] only permission to embrace its conclusion" (IH,263). Again, the constructions of the historian are not arbitrary; they must involve "nothing that is not necessitated by the evidence" (IH,241). As he says on the previous page: The "act of interpolation . . . is necessary or, in Kantian language, *a priori*". If I am not mistaken, this is



something Mr. Donagan has not paid very much attention to. All he does is to tell us (D,196) that "whatever the nature [of the inference] may be (on this question he is silent) it is compulsive". No wonder he thought Collingwood silent on this question!

I hasten to explain. When Collingwood claims that historical inference is necessary, he does not of course mean that whatever an historian writes down is necessarily true. On the contrary, Collingwood especially remarks that "in practice, this aim can never be achieved . . . this separation between what is attempted in principle and what is achieved in practice is the lot of mankind" (IH,247). Similarly, to tell an historian that he must grasp the thoughts behind the events isn't to say that he must be successful or even ever *can* be so, completely—although he is nonetheless still telling him to try and do it! Mr. Donagan indeed takes special notice of this corrigibility proviso (D,200) but uses it to prove that when Collingwood preached that "the re-enactment of past thought is not a precondition of historical knowledge, but an integral element in it" (IH,290) he meant that phrase to be understood as indicating a goal towards which an historian strives, and not part of his method. I should have thought that the phrase could equally well bear the directly opposite interpretation. Compare the following: (1) the historian must try to grasp the thought behind the event; (2) the historian must succeed in grasping . . . ; (3) the historian must in principle be capable of succeeding. What Mr. Donagan's remark suggests is that the corrigibility-proviso must entail that Collingwood was interested only in (3), whereas I should have thought it was more at home with (2). And (2) of course has the ambiguity previously referred to: it looks towards both (1) and (3).

It may be useful to compare this with another celebrated methodological necessitarian account, that of Aristotle, in the *Posterior Analytics*. Aristotle there says that "*We suppose ourselves to possess unqualified scientific knowledge (of a fact) . . . when we think that we know the cause of that fact . . . and, further, that the fact could not be other than it is*". (Italics mine.)<sup>8</sup> To know that the fact could not be other than it is, you must intuit the Universal, a feat in which you succeed when it is "stabilized in its entirety within the soul".<sup>9</sup> Now notice that the italicised phrases clearly indicate that the account is an hypothetical one; there is no suggestion, so far, that we

<sup>8</sup> *Posterior Analytics*, I:1:71b8-12.

<sup>9</sup> *Posterior Analytics*, II:19:100a7-100b3.

can ever know for certain that we have knowledge of what is certain. To this extent, the advice to "stabilize the universal within the soul" is the expression of a goal; when we have done so, we may be *said* to possess knowledge. But surely, what is implied is that there is something corresponding to a *process* of stabilisation, something one might call "the tactics and strategy of science"—indeed Aristotle likens it to the stopping of a "rout in battle"!—to a "process" of "establishing" the required concepts by a pervasive survey of particular instances.<sup>9</sup> Of course, in a sense there is no such process, or if there is, it is simply the application of our usual inductive procedures and perhaps the "intuition" that certain concepts of a field of science are fundamental, e.g. the concept of circular or uniform straight-line motion. But it is just because the phrase "stabilising the Universal within the soul" hasn't yet *got* a grammar—though we may try to philosophise about it—that this ambiguity is possible. Not having a grammar, the phrase, as such, would be useless, or at best the expression of a pious hope, or a tautological definition of "scientific knowledge". One may say: When used as a justification of scientific reasoning, it cannot have a grammar; and in so far as it has a grammar, it cannot act as a justifier. And only an artful combination of both, a metaphysical double vision, can constitute anything of interest for a philosophical account. Just so in Collingwood's case, as already suggested. The phrase "re-enactment of the past" has the grammar (concrete or abstract) that emerges from a careful study of its *use*; but to this extent it cannot act as a justification. Which becomes obvious when we find that we are still expected to justify what has been given as a perfectly good *descriptive* account of the process. When in turn we try to give a justification by beginning "seriously" to investigate the grammar in terms of "the revival of past activities" we realise that none of these models can ever be allowed to work, just as no "models" of the universe that attempt to give a definition of "lawfulness" can ever work.

I have said that an important clue for the understanding of Collingwood is his insistence on the necessitarian aspect of the interpolating construction of the historian. This feature is the more surprising because Collingwood is so very insistent that the historian's constructions are "autonomous". Mr. Donagan complains that I suggest that Collingwood's autonomous constructions are "licentious" whereas they are said to be something that "involves both making reconstructions and testing them" (D,20). Now it would have been pardonable had I



overlooked this, in the light of those many passages where Collingwood insists that as the historian "becomes more and more master of his craft and his subject, they (his authorities) become less and less his authorities" (IH,238); or that the historian cannot use actual evidence "unless he comes to it with the right kind of historical knowledge . . . Evidence is evidence only when some one contemplates it historically" (IH,246-7). But it is surely a little peculiar to imagine that I would have overlooked Collingwood's constant preoccupation with "evidence". Indeed, at B105-6 I expressly point out that Collingwood distinguishes between the plausibility of a novel and the truth attaching to an historical construction, by means of the fact that the latter has "a peculiar relation to something called evidence" (IH,246). The point is surely that, on Collingwood's account, a source, to become evidence, must run the gauntlet of the historian's critical interpretation; evidence can only be that which can be used as evidence; or, more clinchingly: "There is nothing other than historical thought itself, by appeal to which its conclusions may be verified" (IH,243), for—and ultimately—"the actual evidence is that part of these statements which we decide to accept" (IH,280). Surely, in his desperate keenness to make Collingwood sound more sensible than he is, Mr. Donagan has tended somewhat to underemphasise those features of the doctrine which (though perhaps perfectly sensible aspects of a far more complex story) must be overstressed in order to lead by easy stages to the required metaphysical account. Of course, there is no one who would not agree that we can't have recourse to revelation or intuition (as Collingwood stresses), that all the data, to varying degrees, are "soft" (by contrast with intuition or revelation); or that, with time, the interpretation of the facts of the English Civil War (to mention one relevant example) becomes far more important than the facts themselves. With this we are not concerned. For our concern is with Collingwood the philosopher. We must judge of his philosophical procedure, the relations that exist between his procedural and his philosophical accounts; not whether he sounds sensible or not.

I want to ask: why do we all feel that Collingwood isn't just propounding a bit of commonsense but is trailing a metaphysical coat? Now one hint is, as I have suggested, his provocative suggestion that historical knowledge has a compulsiveness quite superior to that found in the natural sciences, more akin to that of mathematics. Parallel with this is the fascinating remark that to know an historical fact is to know its cause (IH,214).

As I have said, the answer is that out of a deficiency of historical knowledge, as Collingwood sees it, he has made a virtue. History deals with the past. But it deals (and if it wants to be scientific history it must deal) with the *thought* of the past: re-enactment is possible through re-thinking. Now on the face of it, it is not clear why this should be such an important or exciting thing to say. When we look at procedural accounts as examples of what can be meant (e.g. the Theodosius account; the Nelson example; re-thinking Euclid's or Plato's thought), it is not at all obvious how this sort of thing can go any way towards the compulsiveness of a scientific history superior to natural science, akin to mathematics. Certainly the method of sympathetic understanding of another man's thought can be a very powerful aid in "recapturing" the past. But it is by no means the only thing we can be satisfied with; moreover, when that man's thought was incoherent or unmotivated (and the examples from Euclid and Plato deliberately ignore this difficulty), the method isn't likely to be very successful. And, of course, as Mr. Donagan reminds us, it must be constantly checked against "evidence". If Collingwood nonetheless gave such an important place to the method, it must be, not because it was never in any way intended as a piece of method, but because that account *at the same time* serves (or seemed to Collingwood to serve) as a very powerful model in terms of which to satisfy the requirement of scientific history, namely that its "a priori constructions" should be genuinely a priori, namely necessitated. Clearly, necessity cannot be generated by the usual construction-cum-evidence game. But the model for the sort of fact that can become a fact by "being understood",<sup>10</sup> and understood as soon as becoming fact, is thought. To have grasped the full and proper thought of Plato I must not only go through the logical deductions: I must think of it within its proper context. Besides, the model of thought seemed to him to be the only thing answering to the requirement that the past should be capable of being brought back to life. (Cf. incapsulation theory at A69.)

Now it seems to me that we have again here the spectre of the double-edgedness of the requirement of re-enactment, of re-thinking the thought of the past. On the one hand, the requirement that we re-think the thought of the past comes as the answer to the question: How can autonomous construction be compulsive? True, at IH,241, Collingwood had said that it is "necessitated by the evidence", but we know already that

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<sup>10</sup> I.e. necessarily related to its neighbouring thoughts.



evidence is only accepted when satisfying the (necessary?) construction: "The a priori imagination . . . supplies the means of historical criticism as well" (IH,245). Or again, the touchstone will be the coherence and continuity of the picture, "whether it makes sense" (ibid.). However, at IH,282-3, we find that criticism of the evidence in this way is only possible if the historian re-enacts the past in his own mind. I say that this is a procedural piece of advice, for it seems perfectly clear that Collingwood would never have been satisfied with a "method of continuous approximation". But it is thin advice, as just pointed out. It is because it is so thin that people like Mr. Donagan feel that it cannot be meant seriously at all. It is because of this that they want to say that all Collingwood means is: "Do your job carefully and when you have been successful in this, why, then you are re-thinking the thought of the past". To say the least, this would not take account of Collingwood's worry that the *connections* between his facts had to be necessary ones. Whereas he felt that the thought-model was a particularly powerful practical tool whereby to simulate this necessity; without this, it would not be a very exciting thing either.

Perhaps we may distinguish in the following way:—

To be sure, re-enactment or re-thinking, i.e. the autonomous constructions, are neither licentious nor intuitive. They are required to make history possible, but in the following three senses:—

(a) *Weak sense*:

Make history, in any sense of that term, possible.

(b) *Strong sense*:

Make history, in the sense of "genuinely necessary" constructions, possible.

(c) *Intermediate sense*:

Make the perception of connections possible.<sup>11</sup>

But, of course, the model won't do everything all at once. One may bring this out by saying that we feel strongly that the command "embody the thought of the past" cannot be seriously

<sup>11</sup> It is instructive to compare this situation with what we find in Kant. The Principles of Experience make it possible that—

- (a) I should have any experience at all (cf. arguments in the Second Analogy),
- (b) I should be able to have empirically necessary propositions like the Law of Conservation of Mass or the Law of Inertia (cf. arguments in the *Prolegomena* and the *Metaphysische Anfangsgründe*),
- (c) I should (through the empirical synthesis of the manifold) come to a perception of a plurality as unity, e.g. perceive a line as drawn in space.

obeyed. We cannot ask a man to re-live the past in the way in which we ask him to read the inscription on a stone and to interpret it. But of course, only if you could do this, would you have the simulacrum of a structure guaranteeing the necessity of Collingwood's a priori constructions. No wonder our critics felt that it was absurd to suggest that Collingwood had ever meant this! I want to say that Collingwood's interest lies in having gone in for the time-honoured procedure of wanting to have it both ways at once. The very burning piece of advice to try and empathetically understand the past is meant as a procedural method of arriving at as much truth as is possible. At the same time, only if to the *justifying phrase* "re-enacting the thought of the past" there corresponded a methodological procedure (i.e. if it made sense to say: think the correct thought of the past) could the procedural account *also* be a justifying one. But, of course, it doesn't make sense to have it both ways. However, to take Collingwood to have said merely that when you have *done* what would be done on *any* account of history (including working with scissors-and-paste), and done it correctly, then you will have in fact re-enacted the thought of the past, is indeed to propound a doctrine which would be a complete denigration of all the charm there ever was in Collingwood.

To sum up this part of the argument. Mr. Donagan interprets as follows: To do scientific history you must do something, i.e. re-enact the past. But to Collingwood this doesn't literally mean following a special sort of method, e.g. intuition etc., though it may mean employing empathetic understanding; rather, what it means is: you must try and succeed in having the thought of the past before your mind. And again, to try and succeed to have the thought of the past before your mind doesn't mean you go in for any special sort of motions, but you do whatever any respectable historian does; and when you have succeeded in doing this as well as you can, e.g. by running the gauntlet of both "evidence" and "theory", then you will have before your mind the thought of the past.

Conversely, and this is important, unless it were possible to have the thought of the past before one's mind, there would be no guarantee that history was possible, i.e. that doing the sorts of things every historian does would ever eventually lead to history either in the usual sense—weak sense (a)—or the strong sense (b), i.e. to scientific history, i.e. compulsive conclusions.

We may well ask: on this account, what would be the connection between doing what every respectable historian does,



and the requirement that success can only be assured if as the result of the process I have the thought of the past before my mind, where—be it once more remarked—*ex hypothesi* the process need have nothing to do with the re-enacting or anything of this sort?

Compare this once more with the Aristotelian argument. To reach scientific conclusions, we must stabilise the Universal. But this doesn't mean: go and set yourself to do anything, e.g. intuit a universal; rather do whatever you normally do by way of induction and when you have done this as well as can be expected of mere mortals, you will in fact have established the Universal; you will in fact have scientific knowledge; for unless you could stabilise the Universal, there would be no guarantee that you could ever reach *bona-fide* scientific knowledge. I need not stress that the connection between the process and its justification is here even more tenuous, owing to the fact (as before remarked) that "stabilising the Universal" has barely a grammar at all. And this has the result that it becomes more obvious that the justification phrase acts solely as a justification, i.e. an empty insistence that scientific knowledge is possible, i.e. that the phrase "scientific knowledge" is significant. And I have argued that Collingwood's justification phrase ("re-enactment") *qua* such a phrase, must lack significance in a similar way. If all the same it doesn't, this is because it borrows fine feathers from the methodological grammar.

On Mr. Donagan's account there is absolutely no connection between the two; and however edifying it may be to have a *definition* of "scientific history", this would not bring us one whit closer to the very real troubles which Collingwood had felt to beset his account of the *a priori* constructions of the historian. But that he thought his requirement that the historian should re-enact was more than an expression of a definition of the possibility of scientific history, and was meant to be at the same time a piece of methodological advice, becomes absolutely clear if we remember Collingwood's argument mentioned a moment ago. Criticism of the evidence is only possible if the historian re-enacts the past in his own mind (cf. IH, 282). It is here, to repeat once again, that lies the sting of Collingwood's philosophical procedure. Re-enactment on the one side presents itself as a harmless subsidiary tool, of greater or less importance in different contexts; exposed to all the vagaries of the game of testing the evidence against the theory and the theory against the evidence. Its exciting aspect appears only if the suggestion is smuggled in that because (as Mr. Donagan has

so ably shown) it is meant to act as a justifying clause it can add a special sort of compulsiveness to historical conclusions, supplying a special sort of critical apparatus. And *per contra*: the justification account would reduce to some pretty anæmic sort of "grammatical jugglery" unless it drew strength from the concrete models in terms of which the procedural account is described. I do not think Collingwood has been so very unlucky in his critics when they have tried to point out the sort of features belonging to his philosophical work which so brilliantly illustrate the general procedures of those philosophers who have been intent on metaphysics.

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## AYER'S ANTI-PHENOMENALISM

By K. W. RANKIN

In his article on 'Phenomenalism' (*P.A.S.* 1946-47, reprinted as No. 6 of his *Philosophical Essays*) Professor Ayer cites the case of a pen in his possession which vanished without trace. This example and that of a telephone instantaneously changing to a flower-pot are directed against phenomenalism. Allusions to the same sort of example appear more recently in *The Problem of Knowledge* (Penguin ed. p. 129 f.) but with a somewhat elusive change of emphasis. Both his earlier and later applications seem to me to invite reconsideration. But first a preliminary examination of analysis in general may place them in better perspective.

Philosophical analysis by definition is the search for other statements which individually or compounded are logically equivalent to a given statement or analysandum. Accordingly, when viewed as a form of analysis, phenomenalism is primarily concerned with logical relationships. Now analysis as thus understood may be undertaken from one or more of a number of motives. Of these the two of greatest philosophical importance seem to be the metaphysical (or anti-metaphysical) and the epistemological. In certain cases these motives may readily be confused, particularly where the metaphysical significance of an analysis rests upon epistemological assumptions. Thus in carrying out Russell's programme of substituting, wherever possible, logical constructions for inferred entities, one necessarily treats the material of the construction as uninferred or immediately known, otherwise there would be no point in substituting it for the inferred entity. Nevertheless the programme is basically metaphysical in nature. Its purpose is not to show how the presence of a physical object, for example, can be conclusively known, but rather to show, whether its presence can be conclusively known or not, that it is not something metaphysically distinct and separable from what can be conclusively known, viz., the presence of some sense-data. Similarly Ryle's analysis of statements about mental activity into hypothetical statements about overt behaviour has its epistemological implications in so far as it undermines the appeal to introspection or inner sense. Its primary function however

is metaphysical—to show that mind is not something distinct and separable from certain bits of overt behaviour.

Accordingly, when an analysis fails to suggest a satisfactory account of how we know conclusively the truth of the analysandum, that in itself is insufficient reason for rejecting it. From the metaphysical or anti-metaphysical point of view it may be perfect. It is the logical equivalence of the analysandum and proposed analysans which is of primary importance here, rather than the ability of the analysans to act as a premiss from which the analysandum could be derived. For this reason, I maintain that the metaphysical function of analysis is primary and its epistemological function only secondary, since according to my original definition analysis is the search for logical equivalences between different types of statement. Nevertheless metaphysical and epistemological interests may interact legitimately in the following way.

Natural piety towards paradigm cases<sup>1</sup> may lead us to think that any attempt to treat our affirmations about, say, tables, chairs, pens, and hands as unrepresentative of true knowledge must be an abuse of the ordinary word 'know': for as a matter of fact those are just the sort of things about which we are least in doubt. Opposing courses are then open to us. 1(a) We may suppose that if we know the truth of the analysandum (e.g. some statement about tables or chairs) then we must know the truth of the analysans (e.g. some group of statements about sense-data), and further that the latter is logically equivalent to the former. Consequently we will use the paradigm instances of knowledge as touchstones for the analysis rather than the analysis as a touchstone of knowledge. The line we take is that we know the truth of the analysandum and that therefore nothing of which we don't know the truth can be permitted into the analysans.

But from the same original stance we can take another course. 1(b) We can deny that knowledge of such paradigm cases involves analysis at all. On this view, in affirming what we know and knowing what we affirm we detect no logical equivalence between what we affirm as known and what we affirm as the evidence for what is known. We may maintain that to cast doubt on what we know is never *logically* inconsistent with the evidence we have, but that it is inconsistent with the common use of 'know'.

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<sup>1</sup> For more explicit comment see my article "Linguistic Analysis and the Justification of Induction". (*The Philosophical Quarterly*, Oct., 1955, p. 326ff.)



If our interests are metaphysical, on the other hand, quite a different choice lies before us. 2(a) We may, perhaps, treat paradigms of knowledge with scant respect and use analyses which seem metaphysically justified as touchstones for 'true' knowledge. We may prefer for instance to treat tables and chairs as logical constructs of sense-data rather than as inferred entities, and then suspect their reality on the grounds that most of the material from which the construct is built can never be available to us. The line we take is that we don't know the truth of the analysans and that therefore we can't know the truth of the analysandum.

2(b) Alternatively we may studiously avoid committing ourselves for or against the supposed paradigms of knowledge. We may confine the import of our analysis to metaphysics alone. Our concern, we may say, is not to find logically sufficient evidence for what we say we know, but merely to decide whether there can be any logical relation of equivalence between what we say we know (e.g. facts about tables and chairs) and facts about their evidence (e.g. sense-data). We wish to know in other words whether reality consists of anything more than sense-data and their logical constructs.

Now in what way do these preliminary observations on analysis in general apply to Ayer's criticisms of phenomenalism? In both his earlier and later discussion he accepts as "the lively testimonials" of our knowledge just what in fact we ordinarily take to be the certainties of sense-perception, and never once deviates from this orthodoxy. In "Phenomenalism", however, he takes the very first of the courses, viz., 1(a), which this stance permits. To meet with any success the phenemonalist must, he thinks, supply an analysis of the facts which we perceive, and by this analysis he must vindicate the certainty of these facts. He assumes that to know the truth of an analysandum is to know the truth of the analysans and to detect the relation of equivalence between the two. If the certainty with which we know the analysans were less than that of the analysandum, then phenomenalism would be a failure. Here, then, the paradigm instance of knowledge is used as the touchstone for the phenomenalist analysis.

In *The Problem of Knowledge* on the other hand, his procedure is different and less legitimate. He takes the second course left open from the same stance, viz., 1(b). He retains the same paradigms of knowledge but rejects the assumption that to know something about the physical world is to detect a logical equivalence between an analysandum and an analysans.

The known facts about the physical world are no longer viewed as an *analysandum*, nor the evidence for these facts as their *analysans*. This new course might be justified were it not intended as a criticism of phenomenism. Whether the facts about the physical world are paradigms of knowledge or not, the phenomenist certainly need not suppose that knowing these facts consists in detecting a logical equivalence between them and their evidence. He can take the epistemologically neutral course (2(b)). Instead of offering his analysis as an account of how we know facts about the physical world, his purpose might simply be metaphysical.

I shall now follow and comment upon his arguments in greater detail.

In "Phenomenism" he argues<sup>2</sup> that if a telephone suddenly changes to a flower-pot, or a pen unceremoniously disappears, we wouldn't necessarily suppose that there never had been a telephone or a pen respectively. He is using a paradigm for knowledge even though the occurrences which he cites are infrequent. They help us presumably to clarify what elements in paradigm instances of knowledge are essential or inessential to their being such instances. The immediate conclusion they suggest to him is that a finite number of sense-data provides sufficient evidence for the existence of a material object, and that, contrary to certain allegations, phenomenism does not for ever postpone our right to logical certainty about certain simple physical facts. By further examples he shows<sup>3</sup> that this evidence is sufficient but not necessary. The evidence is not necessary because a different set of past experiences could conceivably have satisfied him of the existence of the telephone or pen. On these grounds he claims that "it now turns out that, for reasons I have given, statements about physical objects cannot be translated into statements about sense-data".<sup>4</sup>

His argument here gives rise to two queries. First, if a finite number of sense-data really does provide sufficient but not necessary evidence for the existence of a material object, why should this embarrass phenomenism? Second, do his examples really support his claim that a finite number of sense-data provides sufficient but not necessary evidence for the existence of a material object? To take these questions in order. Ayer seems to be operating with an over-simplified conception of

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<sup>2</sup> *Philosophical Essays*, pp. 135-38.

<sup>3</sup> *Op. cit.*, pp. 138-40.

<sup>4</sup> *Op. cit.*, p. 142.

analysis. Analyses *may*, it is true, consist of a statement or conjunction of statements finite or infinite. Where this is so each statement in the analysans must be a necessary condition of the analysandum and only the complete analysans can be a sufficient condition. Accordingly if a finite number of sense-data provides sufficient but not necessary evidence for the existence of a material object, we must reject any phenomenalist analysis which conforms to this first pattern. But there are other patterns of analysis. For instance, instead of being conjunctions, analyses may consist of disjunctions of statements or disjunctions of conjunctions of statements. Furthermore, both the disjunction and the conjunction may be finite or infinite in extent and there may be greater or lesser overlap between the disjointed conjunctions. There are in other words a variety of analytical patterns which the phenomenalist might adopt. If he adopts a disjunctive-conjunctive pattern of analysis he can agree with Ayer that a finite number of sense-data provides sufficient but not necessary evidence for the existence of a material object. If an analysis consists of disjunctions of finite conjunctions then the truth of each of the conjunctions is a sufficient but *not* necessary condition of the truth of the analysandum.

In spite of this Ayer might compel us to agree with him when he says that "statements about physical objects cannot be translated into statements about sense-data". It all depends on what weight he is prepared to place upon the phrase "cannot be translated into". When an analysis becomes sufficiently complex of course it cannot be used for the purpose of translation. If, for instance, the extent of the disjunction of conjunctions is very large or infinite it becomes useless for purposes of translation. But the metaphysical (or anti-metaphysical) function of an analysis is not that of translation. An analysis must provide a translation only where we subordinate its metaphysical to its epistemological function.

Ayer might perhaps seem to concede this point, for he allows the phenomenalist to modify his position by saying that when you speak about physical objects "what you are saying, though vague, still refers ultimately to sense-data and does not refer to anything other than sense-data".<sup>5</sup> He is indeed on the verge of concession, but his use of a deprecatory term like "vague" conceals the fact that the primary function of phenomenalism is metaphysical. For this purpose actual translations of statements about physical facts are quite otiose

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<sup>5</sup> *Op. cit.*, p. 142.



once the phenomenalist has made his point that there is no inferred entity underlying the appearances. Consequently, since the metaphysical purpose of analyses is not translation, there is no question of their being either precise or vague from this point of view. The most we can say is that they either do or do not consist of disjunctions of statements and accordingly will or will not yield vague translations if used for epistemological purposes.

Now for my second question. Do Ayer's examples really warrant his conclusion that a finite number of sense-data may supply sufficient if not necessary evidence for the existence of a physical object? One must here clearly distinguish between failure to disprove the past existence of a telephone or pen in the light of further evidence and a successful proof of their existence. Not to disprove is not to prove. All that Ayer's examples have shown, it might be argued, is that the unanticipated presence or absence of sense-data does not necessarily entail that the supposed physical object never existed; they do not show that the finite number of sense-data prior to the unanticipated sense-data is the sufficient condition of the physical object's existence. On this argument Ayer's examples are consistent with a rather different sort of phenomenalism. I have already described a phenomenalist type of analysis, consisting of a finite or infinite disjunction of finite conjunctions, which was consistent with Ayer's interpretation of his examples. But I am now suggesting that a phenomenalist analysis consisting of a finite or infinite disjunction of *infinite* conjunctions would be more adequate. According to this type of analysis only an infinite number of sense-data can provide sufficient, though not necessary, evidence for the existence of a physical object.

Suppose I have the sense-data commonly associated with the existence of a telephone or pen and with the sort of behaviour in other people which confirms the existence of a telephone or pen. If we suppose with Ayer that these are suddenly followed by sense-data commonly associated with the existence of a flower-pot or non-existence of a pen, and with the sort of behaviour in others which confirms respectively this existence or non-existence, we may well agree with Ayer that we would still retain a belief in the past existence of the telephone or pen. We would retain this belief particularly if the sense-data associated with the behaviour of others led us to believe that they too had memories of the sense-data associated with a telephone or pen. But now let us further suppose that we had the sort of experience we call "waking up". One important

constituent of this would be that the sense-data of the behaviour of others would not lead us to suppose that they had memories of sense-data associated with telephones and pens. In such circumstances we would reject the belief in the past existence of these physical objects. Now at any stage in our experience it seems possible that we may have the experience of waking up. Accordingly no finite amount of sense-data ever really does provide sufficient evidence for the existence of a material object.

Take the familiar tale of the girl who with her mother visits Paris on the eve of an exhibition. During the night her mother vanishes without trace. Railway-porters, taxi-drivers, reception-clerks, even the hotel-register, all testify to her having arrived unaccompanied, in contrast to what their behaviour testified on the previous day. Can one then really say that the girl had had sufficient evidence to justify the conclusion that she had not been dreaming or suffering from hallucinations? Or does the evidence unequivocally point to an explanation like the real one, viz., that her mother had inconvenienced the tourist trade by succumbing to bubonic plague?

For myself I am not certain. Ayer might have said that she had sufficient evidence of her mother's presence the previous day to convince her if only she had remembered and assembled it properly or hadn't allowed the authority of the hotel-manager and prefect of police to undermine her self-confidence. But even if the sense-data are there in the past whether we avail ourselves of them or not, are they there in any unequivocal sense? Are they there, for instance, in dream time or in physical time? If in physical time, isn't their thereness at stake along with whether they belong to actual physical objects? It is only as appearances of physical objects that sense-data occupy a position in physical time. Consequently to suppose that they are there in physical time is to beg the question of their sufficiency as evidence for physical objects. However, whatever the answer to this question may be, some form of phenomenalism remains consistent with it.

In *The Problem of Knowledge* Ayer's strictures on phenomenalism betray similar defects. Both his patterns of analysis and his conception of the functions of analyses, whatever their patterns, are unduly restrictive. These defects, however, show themselves in ways different from before. He writes:<sup>6</sup>

"If the phenomenalist is right, the existence of a physical object of a certain sort must be a sufficient condition for the

<sup>6</sup> *The Problem of Knowledge*, pp. 124-5.

occurrence, in the appropriate circumstances, of certain sense-data; there must in short be a deductive step from descriptions of physical reality to descriptions of possible if not actual appearances. And conversely, the occurrence of the sense-data must be a sufficient condition for the existence of the physical object; there must be a deductive step from descriptions of actual, or at any rate possible, appearances to descriptions of physical reality. The decisive objection to phenomenalism is that neither of the requirements can be satisfied."

This statement of the two requirements is correct enough if we dismiss the epistemological overtones of the phrase "deductive step": the phenomenalist need not believe that any deductive step is possible, in so far as that depends upon our limited capacity of comprehending all the relevant facts. Ayer's arguments that these requirements cannot be satisfied are on the other hand largely irrelevant. His argument against the first part of phenomenalism, that there must be a deductive step from descriptions of physical reality to descriptions of possible, if not actual, appearances, depends once more upon an over-simplified pattern of analysis. The gist of his argument<sup>7</sup> is simply that descriptions of physical reality entail no specific conjunction of statements about appearances. But the same reasons suggest that they entail a disjunction of conjunctions of statements about appearances. And this he ignores.

However, my main concern is with his argument against the second requirement, that there must be a deductive step from descriptions of actual or at any rate possible appearances to descriptions of physical reality. He begins from the position that no descriptions of a *finite* number of appearances, actual or possible, logically entails a description of physical reality. He writes:<sup>8</sup>

"At the present moment there is indeed no doubt, so far as I am concerned, that this table, this piece of paper, this pen, this hand and many other physical objects exist. I *know* that they exist, and I *know* it on the basis of my sense-experiences. Even so, it does not follow that the assertion of their existence, or of the existence of any of them, is *logically* entailed by any description of my sense-experiences."

He here retains the same sort of paradigms of knowledge as in "Phenomenalism", but no longer believes that what he

<sup>7</sup> *Op. cit.*, pp. 127-33.

<sup>8</sup> *Op. cit.*, pp. 126-7 (my italics).



now calls sense experiences can ever supply logically sufficient evidence for these paradigms, thus retracting his earlier opinion. It seems then that he has come round to the view 1(b) that knowledge of these paradigms does not involve analysis of any sort: for he admits that this knowledge is based on the evidence of sense-experience, but no longer accepts any logical entailment between this evidence and what it establishes.

At the same time he fails to realise that thereby he has disqualified himself from the contest for or against phenomenalism. The phenomenalist is, as we saw earlier, at liberty to follow line 2(b), avoid commitment for or against any paradigms, and confine himself simply to the question whether statements about physical objects are logically equivalent to anything more than statements about sense-experience. Instead Ayer seems to think that the phenomenalist remains committed to course 1(a), i.e. to vindicating the paradigm cases of knowledge by means of his analysis.

He asks how the phenomenalist could show that there is no logical possibility of explaining away the finite evidence for the pen etc., and concludes:<sup>9</sup>

"For the phenomenalist to succeed, he must be able to produce a specimen set of statements describing the occurrence in particular conditions of certain specified sense-data from which it follows logically that a given physical object exists. And I do not see how this is to be achieved."

But why foist this impossible task upon the unfortunate phenomenalist? Apparently because in "Phenomenalism" Ayer thought he both could and should fulfil it. Ayer has now had doubts about whether the phenomenalist can fulfil the task but doesn't reconsider the question whether he is really committed to trying. In fact, as I hope I have sufficiently emphasised, the phenomenalist is not committed to trying. An analysis may have a metaphysical function without having any epistemological commitment, and it must have a metaphysical function whatever its epistemological commitment.

His restrictive conception of the function of analysis explains further why Ayer's own positive contribution to the theory of perception in *The Problem of Knowledge* seems so unsatisfying. He writes:<sup>10</sup>

"In the end, therefore, we are brought to the unremarkable conclusion that the reason why our sense-experiences afford us

<sup>9</sup> *Op. cit.*, p. 127.

<sup>10</sup> *Op. cit.* p. 132.

grounds for believing in the existence of physical objects is simply that sentences which are taken as referring to physical objects are used in such a way that our having the appropriate experiences counts in favour of their truth."

Why is the unremarkability of this conclusion worth remark? Simply because Ayer has not clearly shown that the question it directly answers is itself unremarkable. If we ask "Under what conditions do we normally say that a physical object exists?" we don't hesitate long for an answer. But why equate this tame question with the problem of perception? It becomes necessary to do so only on one assumption. No other question remains with which to identify the problem of perception, if we regard the metaphysical problem of the relation between physical objects and sense-data (or the logical equivalence of statements about these two types of things) as irrelevant to the problem how we know about the existence of physical objects.

But this assumption begs the major question, viz., "Should we prefer 1(a) and 2(a), as conceptions of analysis, to 1(b) or 2(b)?" According to the attitudes defined in 1(a) and 2(a), analyses do have epistemological as well as metaphysical commitments. The first respects the paradigms of knowledge and trims the analyses accordingly, whereas the second does something like the reverse. According to the attitudes defined by 1(b) and 2(b), on the other hand, analyses have no epistemological commitments or, if they have, so much the worse for them. It is these two alone which lead naturally to the tame interpretation of the question raised by Ayer, viz., "why our sense-experiences afford us grounds for believing in the existence of physical objects". Consequently his answer is not a conclusion to a discussion of the problem of perception. It is only an appendix to the conclusion he probably would have drawn if he had ever successfully raised the major question. However, he never clearly recognises that analyses have a metaphysical function whether or not they have an epistemological commitment, and so cannot clearly raise the question what their epistemological commitment is.

The main issue, as I see it, is this. Must we recognise entities which are independent of the evidence from which they are inferred? If not, then some form of phenomenalism seems called for. If it is called for, would it seek to give logical certainty to what normally we think we know? To this we may answer "no" with the possible proviso that it should not entail that we know something of which we are definitely ignorant. On the other hand we may answer "yes", and immediately run

into danger. If phenomenalism fails utterly to give logical certainty to what we normally think we know, then we have either to throw it over and embrace inferred entities, or to doubt systematically what we normally think we know. This is how I think the metaphysical issues are linked up with the epistemological. Certainly more is at stake than Ayer's question "why our sense-experiences afford us grounds for believing in the existence of physical objects" seems to indicate.

I have commented solely upon those criticisms of phenomenalism which Ayer has pushed with most confidence. It may be vulnerable to some of the others employed by him. However, my purpose has not been to defend phenomenalism but to reconstitute it as a metaphysical problem.

University of Malaya.



## DISCUSSION

### FEELING-STRIVING AND OBLIGATION

By L. E. PALMIERI

Usually there is some meeting in a philosophical exchange. And just as frequently it can be presumed that the readers benefit, however the argument goes and regardless of the party determined to be in error. Unhappily this is not the case between Professor Garnett and myself, considering my *The Feeling-Striving Process*<sup>1</sup> and a short paper to which he gives the same title and which appeared in the December, 1957 issue of this *Journal*. Indeed, a perusal of Garnett's latest effort should cause the reader some puzzlement, for, though it is intended as a reply, he has missed the very line of my progression. More than this, the line of argument he would impute to me is very silly. Under the circumstances I offer a short restatement.

I *allowed* that the five points of evidence which Garnett gave in *The Moral Nature of Man* were creditable support for the claim that there are such things in the universe as feeling-striving processes. However, noting that his motive was to give a naturalistic explanation of obligation which would be adequate and free from what he took to be difficulties in the views of other naturalists when faced with the same problem, I went on to assert:

"The difficulty is to go *beyond* and establish, by good or conclusive arguments, that 'the *primary tendency* [italics mine] of the feeling-striving process as a whole is a disinterested striving to produce what seems to the individual objectively good' and that the processes 'can only constitute an integrated whole as far as they are directed objectively toward what appears to be the greatest possible good'" (p. 56).

In short, the question is not whether there is a feeling-striving process—a legion of Empiricists admit some such process, including those of such different emphasis as Dewey and Hume—but, rather, where lies the evidence for claiming that the *primary* aim of my feeling-striving process is the producing of what seems to me objectively good. And I said so.

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<sup>1</sup> This *Journal*, v. 35, no. 1, pp. 54-59.

To continue, judging it quite apparent that no attempt was made to offer argument showing that the production of what seemed to the individual objectively good was the primary aim of the feeling-striving process—though it appears that Garnett has a fine talent for using “because”, “thus”, “therefore” and the like without intending argument—and yet keeping in mind that he was addressing himself to philosophers who could not, even if sympathetic, allow him to fasten all sorts of new properties to the *admitted* feeling-striving process, I reported my belief that his own easy use of such expressions as “to increase the malleability of the environment to life activity” committed him to positing an ubiquitous Feeling-Striving Process. It seemed to me—and it does still—that language had entrapped him.

The reader can share my astonishment when Garnett now replies that he never intended to prove the existence of the feeling-striving process but only to sketch in evidence. (Cf. p. 212.) The astonishment grows when one continues to read:

“Dr. Palmieri, however, goes on to develop a second criticism. He argues that *since* [italics mine] Garnett’s empirical evidence, here presented, is insufficient to prove his point, he must, in his own thinking, have arrived at that point by a semantic confusion . . .”

Of course, nothing could be further from the case. Not only did I not argue that *since* Garnett does not make his point he *must* have come to the conclusion by a semantic confusion, but, what is worse, Garnett still labours under the belief that my crucial point, and the one moral philosophers would find interesting, was to cast doubt on the existence of feeling-striving processes. Or, even, that *he* had not proved the existence of such processes.

It is a little disconcerting to discover that a philosopher who would appeal to the rational in man to answer many ethical problems is so cavalier with reasoning and argumentation.

Chicago, Illinois.

## ON NOT KNOWING WHAT ONE IS SAYING

By ROBERT AMMERMAN

While engaged in defending his well-known thesis concerning dreams and scepticism against Brown's criticisms, Norman Malcolm contends that:

"A man talking in his sleep could make the sounds, 'I am overdrawn', but he would not be *saying that* (claiming that) he is overdrawn. For the latter it would be necessary that he should know that he was speaking and know what he was saying, and if he knew these things he would not be *talking in his sleep*."<sup>1</sup>

I propose to show first that Malcolm's conception of what is necessary for saying that (claiming that) is mistaken. I will then state an alternative view concerning what conditions must be satisfied if one is to claim something to be the case.

Malcolm evidently wants to hold that one must both know that one is speaking and know what one is saying in order to say that *p*. If these were the necessary conditions of saying that *p*, it would follow that a person could not think that he was saying one thing and say another; for, if he is wrong about what he thinks he is saying, he doesn't know what he is saying and according to this view he is consequently not saying anything. A person who said something other than what he thought he was saying would at best be uttering sounds like a parrot and would not be making any claim at all. This consequence of Malcolm's thesis, I submit, is difficult to accept.

There are indeed many ways of saying something when one doesn't know what one is saying. We sometimes have occasion to use the expression, "I didn't know what I was saying", and often we mean that we did not know at the time of saying that *p*, the consequences of saying that *p*. ("I never would have said it, if I had known she would take it that way.") This way of claiming but not knowing what one is claiming is not, however, incompatible with Malcolm's proposed necessary conditions; for, although one may not have been aware when saying that *p* of the consequences of saying that *p*, nevertheless one may well have known at the time that one was saying that *p*.

<sup>1</sup> "Dreaming and Scepticism: A Rejoinder." *This Journal*, Vol. 35, No. 3, December, 1957.



A second way of saying something when one doesn't know what one is saying is more germane to the issue and constitutes a refutation of Malcolm's position. A person might claim that a certain pillow is coloured mauve, thinking that the word "mauve" refers to a shade of green. Such a person would be misapplying the word if he used it to describe a green pillow and, in a sense, he would not know what he was saying. Does it follow, however, that such a person would not have said (claimed) anything or that he would merely have been uttering words like a parrot? He would have failed to satisfy one of Malcolm's necessary conditions for claiming, to be sure, but nevertheless he would still be making a claim, although admittedly a false one. If Malcolm's thesis were correct, it would be impossible to make a *linguistic* mistake when claiming anything to be the case, for the misapplication or misuse of a word would automatically entail that no claim was being made. In point of fact, however, there are at least two ways of being in error when one is making a claim. One can fail to speak the truth because one is factually mistaken or because one uses the wrong words.<sup>2</sup> When either or both of these conditions obtain, one ends up saying something that is false, but to say something that is false is surely to say something.

The important distinction which is relevant here is between saying something and meaning to say what one says and saying something when meaning to (intending to) say something else. A person has not failed to say (claim) anything merely because he has said what he didn't mean for lack of an adequate vocabulary. The person in the example above does not, in a sense, intend to say that the pillow is mauve, although, in another sense, he does mean to say it. The person does intend to *use* the word "mauve" to describe the pillow because he thinks it is the correct word to use, but by using that word he says something other than what he intends to *say*, and hence, doesn't know what he is saying.<sup>3</sup> The fact that words are part of a common language and have relatively fixed meanings which are independent of our individual uses often leads us to claim and say what we don't mean to say.

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<sup>2</sup> Some philosophers have contended that there are certain propositions—the so-called "basis propositions"—concerning which we can never be non-linguistically mistaken. If this view is correct, our claims concerning appearances or sense-data would be an exception to this general rule.

<sup>3</sup> There are occasions when we say something other than what we intend to say and know at the time of speaking that we don't intend to say what we are saying. (e.g. "I'm not expressing myself well. I don't really mean to say that.") There are other occasions when we don't realize that we are saying what we don't intend to say, and it is only these cases which are relevant to the present discussion.

My objection to Malcolm, then, can be summarized in this fashion. We can distinguish the following cases: (i) saying  $p$  like a parrot, when the words have no meaning for the speaker; (ii) saying that  $p$ , meaning to say that  $p$ , and knowing that one is saying that  $p$ ; (iii) saying that  $p$  when one meant to say that  $q$ , but not realizing that one is saying that  $p$  because one thinks that to say that  $p$  is to say that  $q$ . If instances of case (iii) do occur, as surely they do, then knowing what one is saying cannot be a necessary condition of saying that (claiming that) and Malcolm's position is untenable.

It remains for me to sketch the conditions I believe to be necessary in order to say that (claim that). They are three in number. First, one must know that one is speaking. Secondly, one must intend to say (claim) something, even though it may turn out that one didn't intend to say what one said. Finally, one must intend either to express a belief or tell a lie, even though one may not believe or disbelieve what one does say if one says something other than what one intends to say.

The third condition may need some defence. We sometimes say things to people in jest and often they misinterpret our remarks and take us seriously. In such contexts one can properly deny that one has claimed anything by denying that one meant (believed) what one said. To say, "I was only joking when I called you a fool" is to deny that you claimed earlier that the person was a fool. There is a close connection between meaning what one says and saying (claiming), although it is not so close that one cannot claim something one doesn't mean.

Nothing I have said so far constitutes an objection to Malcolm's main thesis concerning sound sleep and experiencing. On the contrary, to some extent my remarks should tend to bear out Malcolm's view. Unless a person who is uttering words while sound asleep does so intentionally, he is not claiming anything. It is our common practice, however, not to take seriously the babblings of people who are sound asleep, dead drunk or delirious, since we assume that they do not intend to say anything when they are in those states.

University of Wisconsin.

## A NOTE ON THE MEANING OF CONTEMPORARY LOGIC

By R. GEORGE OVEREND

J. P. McKinney<sup>1</sup> claims that recent theories in physics imply phenomenalism, imply that the world is no more than a logical construction from particular experiences; and this, he says, has led the whole of modern thought into a giant logical dilemma. Which is bad enough, but he adds that the school of logical analysis endorses the position.

I shall leave the purely physical question alone but this type of view of recent logic is all too common and it is time someone pointed out that its premises are fictitious.

The logician today is fast becoming a pure analyst; metaphysics is now, by choice, outside his province. How the world goes round, or whether it does, is not his concern, much less to take it to pieces. We have not got a school of empiricists, positivists, behaviourists, phenomenologists: in the main, it is a school of analysts, more akin to lawyers than to anything else. If you imply A and not-A in the same breath and I point it out to you, have I said anything factual? Neither has the logician setting out the implications of physics or mathematics. And if you at the one time assert particular criteria of meaningfulness and go around writing about a world of negative facts that could not be verified by your criteria, this is logically ridiculous, but the logic is yours and the analyst who makes the matter explicit brings in nothing of his own.

This is logic today. Unlike its traditional namesake it owes no allegiance to metaphysics: it does not describe or mis-describe the world, it ignores it. It is a very extensive field of modern thought and in Britain alone it dominates a large number of the Philosophy Departments and has at least taken root in the rest. Its meaning is not fully realized even in Britain itself, and much effort goes into producing criticisms of theses that nobody holds. McKinney mentions phenomenalism, but who is a phenomenologist? Even Herbert Dingle regards phenomenalism as a *viewpoint* entailed by physical theories and not as an *account of reality*. Then again, Ryle is often called a behaviourist, but where has he asserted or implied this in the usual (as distinct from his)

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<sup>1</sup> This *Journal*, December 1956.



sense of the term? His *Concept of Mind* is specifically a logic of various theories: the theories are factual (psychological, physiological or metaphysical) but the logic is not. He calls the book a "geography" but the metaphysical critics never seem to see this. Or take his "Categories"—a paper "rather to remove certain obstacles to the exploration" of the problem of categories "than to proffer actual surveys". Yet how often are his categories (he calls them "proposition factors") read into a traditional framework? This despite statements like: "... if asked such questions as Do proposition-factors exist? What are they like? my answer is 'All such questions are ridiculous ...'". His uses of the term are "purely stenographic", purely a shorthand way of analysing particular arguments, any categories being distinctions inherent in the arguments themselves and not read in or added to them.

Ayer, too, is still sometimes supposed to be an old-style "common-sense philosopher" with certain modern ideas about perception; in some textbooks he gets classified still as an empiricist. But nothing he has written of recent years justifies this, as one or two examples will show: He carefully describes even his *Language, Truth and Logic* as a "logical doctrine" concerning the distinction between various types of statement. He defends his *Foundations of Empirical Knowledge* as an analysis of phenomenal statements.<sup>2</sup> Again, his "Basic Propositions" is sometimes taken to be a re-statement of Viennese verification principles, but what is overlooked is that it was written in response to the request of an editor for an example of analytic work;<sup>3</sup> in particular, his aim was to survey the logical possibilities of what could be meant by (a) the claim that "an *a priori* statement is known for certain to be true" (he tries to show that any interpretation of it is logically absurd); (b), the assertion that "a synthetic statement is true" (he tries to show that the logical possibilities reduce to the assertion that a synthetic statement is reducible to sense-data statements). Likewise his *Problem of Knowledge* is purely analytical, is, as he would say, "neutral with respect to particular matters of fact".<sup>4</sup> Then again, he criticizes Hume for talking about a psychological empiricism when, he says, Hume might have meant to be advancing a logical doctrine about empirical statements.<sup>5</sup>

<sup>2</sup> Cf., for example, his reply to Viscount Samuel, *Philosophy*, 1948.

<sup>3</sup> Cf. the Introduction to *Philosophical Analysis*, edited by Max Black.

<sup>4</sup> Cf. Chapter 1.

<sup>5</sup> Cf. his Introduction to *British Empirical Philosophers*.

These are the people who are being accused of phenomenalism, of breaking down the world of common knowledge. They are the founders of a recent "revolution in philosophy", as they call it: a deliberate concentration on the logic of theories as distinct from theories themselves, ontological, perceptual or what you will. The analysis of the language of enquiries "rather than the compilation of a scientific encyclopædia" is, they believe, their most profitable occupation.

Good or bad, these are the facts; the rising generation of logicians has shelved the perennial problems of philosophy, or rather they have chosen to leave them to the scientists and the remaining minority of metaphysicians. So they cannot be charged with creating metaphysical monsters or endorsing Humean impasses; can only be charged with not doing philosophy. The common confusion arises, first, because some of these logicians still insist on calling themselves philosophers: we are caught in the "systematic ambiguity" of the term. On the one hand we have *philosophers* and *scientists* making a direct contribution to knowledge and, on the other, *logicians*, making an indirect contribution by clarifying particular arguments. And the confusion arises, secondly, because the talk of the analysts is frequently, but not always obviously, elliptical. And occasionally they forget the difference themselves.

The pace of contemporary inquiry is so great that all this seems to have been lost sight of. The result is many complex collisions between ideas that do not in fact conflict.

Edinburgh University.

## CRITICAL NOTICE

THE PROBLEM OF KNOWLEDGE, By A. J. Ayer. London, Macmillan and Co. Ltd., 1956. x, 258 p. 29/9 (Aust.). Also in Pelican books, 224 p. 5/6 (Aust.).

Since the seventeenth century a spectre has haunted European philosophy, the deceitful demon of Descartes. We cannot help believing that there is a material world, and that we know something about its present nature, its past history, and even its future. We cannot help believing, also, that there are other minds besides our own. But have we really got any good reasons for believing these things? May it not be that a deceitful demon has implanted these beliefs in us, although they are quite false? For Professor Ayer the exorcism of this demon is the great business of the theory of knowledge, and in his latest book he brings to the task the clarity, the candour, and the intelligence which characterizes the best work in the British Empiricist tradition. Ayer really feels the force of the sceptical arguments, which is what makes his discussion of them so valuable. He is not content with the facile rejoinder, is prepared to play Demon's Advocate when it seems that the sceptic's opponent is making things too easy for himself. Ayer's view that the main work of epistemology is the refutation of scepticism is, I think, narrow and cramping, but it must be conceded that much can be learnt from a thorough and honest examination of scepticism.

In the first chapter Ayer tries to locate precisely the sceptic's point of attack. He begins by asking what it is to *know* that something is true. One answer often given to this question is that a person knows a proposition *p* when and only when (i) *p* is true, (ii) he is sure that *p* is true, (iii) he has good reasons for believing *p*. Ayer would accept the first two, but in place of the third he substitutes the looser requirement that the person must have *the right to be sure* that *p* is true. (His reason for this amendment will emerge. Ayer, of course, is not denying that having good reasons is one very important way of earning the right to be sure.) He points out further that the standards a person must satisfy in order to earn this right are various and complex, often depending on the special circumstances of the case. For instance, a claim to remember something usually entitles the claimant to say he knows it happened. Yet there are all sorts of circumstances in which we think that a



memory-claim is not sufficient to earn the right to be sure: e.g. memories of early childhood. Furthermore, Ayer argues, the particular nature of the standards which we actually use are not part of the meaning of the word "knowledge" any more than the standards of goodness that we have are part of the meaning of the word "good".

Granted these points, we can now understand exactly what the sceptic is doing. He is not raising the doubt whether our beliefs, or certain classes of our beliefs, really satisfy the *ordinary* standards for knowledge. (Somebody who doubted whether astrology was really scientific *would* be doing this.) Nor is he proposing just to alter our usage of the word "knowledge". (Somebody who proposed that we should not use the word "knowledge" unless the proposition known was necessarily true *would* be doing this.) What the sceptic is doing is attacking *the standards which we normally think give the right to be sure*; he is saying that what we ordinarily count as giving the right to be sure does not really give that right. He is no more trying to change the meaning of the word "knowledge" than the moral reformer who attacks our standards of conduct is trying to change the meaning of the word "right".

However, in Ch. II Sec. (i) Ayer points out that not all attacks on our ordinary standards of proof are examples of philosophical scepticism. Philosophical scepticism is distinguished by two closely connected characteristics: (i) it is not dependent on experience, (ii) it involves a quite *general* attack on our standard of proof. Thus, the philosopher who doubts whether sense-perception is ever veridical does not really base his argument on cases where the senses do deceive us. No demonstration of reliability, no careful excluding of the possibilities of error, will meet his argument. Nor will he be concerned to distinguish a class of cases where the senses are reliable from a class of cases where they are not; for he will want to deny that sense-perception ever gives us the right to be sure.

Ayer further distinguishes between two types of philosophical scepticism, illustrating the distinction by reference to perception, but implying that the same sort of distinction exists in other cases (p. 37-38). He first distinguishes between those who maintain that "all perceptions are bound to be illusory" and the moderates who say that "we can never really know that any are not" (p. 37, *Pel.* p. 38). Confusingly, however, on the next page he re-draws the contrast as being between those who assert that it makes sense to say that all our perceptions are delusive, and

those who admit that some must be veridical, but say that we cannot know which these are.

The fact is that in the case of perception, at least, there are four (perhaps more) possible "sceptical" positions that might be maintained, and are regularly confused. *Firstly*, the sceptic may say that all our perceptions are *bound* to be illusory. I think Ayer is right in holding that we can take a very short way with this contention. As he says:

"A perception is called illusory by contrast with other perceptions which are veridical: therefore to maintain that all perceptions must be illusory would be to deprive the word 'illusory' of its meaning" (p. 37).

For the view that perceptions must be illusory implies that nothing counts as a "veridical perception". *Secondly*, a sceptic may admit that a perception could be veridical, but maintain that in fact every perception is illusory. This view is not a very important possibility, because it is not really a sceptical position—for it claims knowledge, even if negative knowledge, about perception. It would in fact be open to the traditional *sceptical* objections. *Thirdly*, the sceptic may say that any or all of our perceptions can be illusory, or can be veridical, but that we cannot really know whether they are or not. It often seems to be assumed that the necessity of a contrast between the terms "illusory" and "veridical" means that this third position can be dismissed equally briefly, for this view implies that it makes sense to say that all our perceptions might be illusory. (I cannot decide whether Ayer thinks this or not.) In any case it is far from clear that the argument holds here. For this view does not assert that nothing would count as a veridical perception—it asserts only that it may be that none in fact are. I can meaningfully assert that no pigs have wings (no perceptions are veridical). Objects are called winged by contrast with unwinged objects (perceptions are called illusory by contrast with veridical ones), so it follows that I must be able to know what would count as a winged pig (veridical perception). But it does not follow that there are winged pigs (veridical perceptions). Of course, there may be further arguments to be brought against this third position. Thus, if it could be proved that a phenomenalist or neo-phenomenalist account of material objects is a correct one, it would follow that not all perceptions could be illusory. For on such a view an illusory perception is (roughly) one that fails to square with the majority of other perceptions. Hence to say all perceptions are

illusory would be like saying that a majority was in a minority. But Ayer offers no such argument at this stage.

*Fourthly*, and finally, a sceptic might admit that some perceptions must be veridical, yet still argue that we can never really know whether any particular perception is veridical or not. Ayer discusses, and rejects, an attempt to take a short way with this position. It might be argued that we can only condemn certain perceptions by reference to certain other perceptions. I can only have reason to think that the colour of a certain object is misperceived if I have reason to think that on other occasions I or others have perceived the real colour of the object. Now, as Ayer points out, this argument does show that it is impossible for the sceptic to bring evidence from perception to support his sceptical position, for such evidence implies that we know some perceptions are veridical. Once the evidence was doubted, all *special* reason for doubting a particular set of sense-impressions would vanish. (Of course, if it is impossible for experience to justify the sceptic it must be equally impossible for experience to refute him.)

But, as Ayer also points out, this argument fails to refute the careful sceptic, because he can still ask whether we really have any good reason to trust the particular perceptions we do trust. He cannot adduce evidence from experience against any perceptions, but this need not stop him asking whether they are really trustworthy. It appears, then, that even if it makes no sense to wonder whether all our perceptions may not be illusory (and I do not think Ayer's argument has ruled out this sceptical position), yet the sceptic may still protest that we can never really know which of our perceptions are the veridical ones. The short way with sceptics fails.

Now one way that one might try to meet the sceptic's challenge would be to find bases for knowledge which are absolutely impregnable. If we can find statements whose truth can be established beyond the possibility of doubt, and if these can serve as grounds for other statements we claim to know, the demon will be laid. (Historically, of course, such a quest for certainty has gone hand in hand with scepticism.) Ayer therefore sets himself to discuss whether there are any such statements. (Ch. II Sections (ii)-(vii).)

Some Rationalists have looked to a priori statements to provide indubitable propositions. Whatever is a priori true, or validly deducible from a priori truths, is really known to be true. Ayer advances two arguments against this view. Firstly,



as an Empiricist, he argues that this would mean that we could never know the truth of any matter of fact, because all statements of matter of fact are necessarily contingent. Secondly, he points out that it is possible to be mistaken even about the truth of a priori statements. It is true that in many cases we have no doubt whatsoever about the truth of such statements, but, as Descartes recognized, they are not logically indubitable. (Rationalists have confused the logically necessary and the logically indubitable.)

The question then is whether we can find indubitability outside of the realm of the a priori. Ayer first discusses Descartes' "Cogito". To say that I am thinking, or that I exist, is not a necessary truth, because it is possible that I might not have been thinking or existing. Yet both statements seem indubitable. But, Ayer argues, these statements are immune from doubt only because they lack any *descriptive* content. If Ayer says "Ayer exists" or "Ayer thinks" the statements become dubitable because the name Ayer implies a certain description (e.g. called "Ayer"), and it would be possible that the speaker was characterizing himself wrongly. But the "I" in "I exist" or "I think" is a mere *demonstrative*. It is the condition of the use of a demonstrative that the object pointed to exists, and so to say "I exist" really conveys no information. Similarly, "I think" gives no description of one's state of consciousness, it is simply a *signal* of consciousness. These statements, then, only achieve indubitability through being *degenerate* forms of statement, that is, failing to describe. "They point to something that is going on, but they do not tell us what it is" (p. 55, Pel. p. 53).

Ayer passes on to consider a third class of statements which have been offered as indubitable, namely, first person reports of present experience (e.g. "I feel a headache", "This looks to me to be red", "It seems to me that is is a table"). These statements are peculiarly interesting for an Empiricist, for one who holds that our knowledge is based on experience will naturally look to these reports as being the indubitable bases of knowledge that will survive the sceptic's assault. Ayer however offers a number of criticisms of the claim that these reports yield indubitable knowledge. In the first place the reports are only indubitable for the speaker, and while the experience is going on. Yet surely, Ayer argues, the sentences "I have a headache", "I had a headache" (referring to the same headache), "He has a headache" (still referring to the same headache), all

express the same statement. This shows that it is not the *statement* that is indubitable. The position must rather be that it is possible to have indubitable *evidence* for the statement, viz., if you are the person actually having the experience. In the second place, even if there is some sort of indubitability here, we are not taken very far, because *ordinary* claims to knowledge that we say are "based on experience" go well beyond such reports.

Yet there does seem to be something indubitable about such first person reports: it is not very easy to see how we can make anything but a *verbal* mistake when we try to tell the truth about such experiences. Nevertheless, this conclusion worries Ayer; and as an Empiricist he is quite right to be worried. He has, I think, the following picture of the matter. There are, on the one hand, our experiences: feeling a headache, an object looking blue to me now, etc. On the other hand there is a quite distinct occurrence, the judgement which registers the experience, this judgement normally taking the form of a statement. But then it seems impossible to Ayer that the judgement must necessarily correspond to the experience. If it must correspond we have a necessary connection between distinct matters of fact, a thing which is unacceptable to Empiricism. Ayer therefore takes the bull by the horns and boldly denies that such judgements are really incorrigible. He takes the case of two lines which are both in my field of view and which are of much the same length. In such cases, he argues, we may well be puzzled not just to say which *is* the longer, but even to say which of them *looks* the longer to me now. Yet I am in no way uncertain about the meaning of the phrase "looks longer than". He concludes that first person reports of present experience are not really indubitable.

I think Ayer has argued quite correctly from his premises. If experience and judgement are distinct occurrences, an Empiricist can give no other answer. But is his conclusion really acceptable? The case Ayer gives is quite persuasive, but does it make sense to say, e.g.: "It looks to him red now, but he thinks it looks to him green"? On Ayer's view it should, yet it is very difficult to say that it is intelligible. As I understand the matter, Wittgenstein, Ryle, and others have been troubled by the same dilemma that Ayer is troubled by, but they have taken the beast by the other horn. They have accepted the incorrigibility of first person reports of present experience, but they have drawn the conclusion that they are not really *reports*,

that such statements do not really function descriptively. Thus, to say "I am in pain" does not report something that is going on, it is more like a linguistic substitute for a groan. To say "It looks blue to me now" is not a report of my experience, but, perhaps, an expression or signal of an inclination to assert that the object is blue. It would follow that "it looks blue to me now" and "it looked blue to me then" could not be equivalent statements, for the latter is a report, which can be true or false. Furthermore, this view would have to analyse the situation where we were in doubt whether to say that one or the other line looked the longer, as being a situation where one was hesitating between two avowals, "A looks longer to me now", "B looks longer to me now", in the same sort of way that we may hesitate between wine and water. There can be no question of comparing the two "reports" with an "experience" to see which fits the better. Now there is no doubt that both Ayer's view and the alternative view face severe difficulties. Yet it seems that an Empiricist will have to choose one side or the other.

But whether Ayer is or is not right here, on neither view do we reach statements which cannot be doubted. On Ayer's view, reports of a person's present experience are statements, but it is possible to doubt them; on the alternative view they are not really statements at all, or at best are degenerate forms, and so the question of indubitability does not really arise. "What we do not, and cannot have is a logical guarantee that our acceptance of a statement is not mistaken" (p. 71, Pel. p. 67). We may now see the point of Ayer's refusal to say that we know a proposition  $p$  only if we have good reasons for  $p$ , substituting instead the looser requirement that we must have the right to be sure of  $p$ . On the former view, since no propositions are indubitable, the good reason for  $p$  must itself be a dubitable proposition which we claim to know; hence it will need to be backed by good reasons, and so on ad infinitum. Our right to be sure of a proposition therefore *cannot always rest on evidence*, although it often does.

At this stage it would seem that the field has been abandoned to the sceptic. There is no solid rock of indubitable statements, hence all claims to knowledge must be left suspended in the air, lacking any foundation. But, as Ayer sees, it is just this rejection of absolute indubitability that *refutes* the sceptic. We save our life only by losing it. For if the notion of an absolutely indubitable yet informative statement is a meaningless one, it follows that in demanding this sort of warrant for our knowledge the sceptic is asking for the impossible. We contrast knowledge



with lack of knowledge, and if he demands that this criterion of absolute indubitability be satisfied before a proposition counts as being known, then the sceptic is really obliterating the possibility of drawing the distinction. It follows that we can get knowledge on lower terms than those the sceptic demands. Just what the terms are has to be determined in particular cases. If I am asked how I know that  $p$  is true, I may be able to point to evidence  $q$ . And if  $p$  is derivable from  $q$  by a valid process of inference, and if  $q$  is known to be true, my claim to knowledge can be upheld. And I may be able to base  $q$  on further grounds  $r$ . But at some point (perhaps even right from the beginning), we may be unable to give reasons—at some point we must assert a statement without further reasons. But since this is logically inevitable it need not take away the right to be sure. It is true, as Ayer points out, there is something that serves in a way as a foundation for knowledge-claims, viz. the observations that people make. But these are not indubitable, and in any case if everything I know had to be backed up by such statements they would have to consist, for the most part, not of actual observation-statements, but rather of statements about what I might have observed *if* I had had the proper opportunities.

Even now, however, as Ayer sees, the main sceptical assault is still to come. For very often we *do* accredit a statement on the basis of others. Now when we do so the passage from evidence to conclusion must be legitimate. Here the sceptic gets another opening. Baulked in his demand that statements we claim to know must be indubitable, or that their grounds must be indubitable, the sceptic may still attack our right to pass from evidence to conclusion. Such sceptical attacks may put in peril, not just many of our claims to knowledge, but even claims to rational belief.

Ayer begins by considering the problem of induction (Ch. II, sec. viii). No doubt because the question has received such extensive attention of late he deals with it briefly. His treatment of it, I think, adds nothing to what has been said already elsewhere and at greater length. (Whether correctly or not I shall not consider here.) He argues that to look for a justification of inductive procedures is an impossible activity; for a deductive justification must be ruled out on Humean grounds, and any inductive or quasi-inductive justification is circular. Once this is seen, we can be content to let scientific method be its own justification. Once again, when the logical impossibility of meeting the sceptic's demands are seen, this is the ruin of scepticism. His case is too good.

We now come to the most interesting and brilliant section of the book (Ch. II, secs. ix and x). It is an exercise in the classification of philosophical theories. Ayer goes on:

"There is, however, a special class of cases in which the problems created by the sceptic's logic are not so easily set aside. They are those in which the attack is directed, not against factual inference as such, but against some particular forms of it in which we appear to end with statements of a different category from those with which we began. Thus doubt is thrown on the validity of our belief in the existence of physical objects, or scientific entities, or the minds of others, or the past, by an argument which seeks to show that it depends in each case upon an illegitimate inference. What is respectively put in question is our right to make the transition from sense-experiences to physical objects, from the world of common-sense to the entities of science, from overt behaviour of other people to their inner thoughts and feelings, from present to past" (p. 81-82, *Pel.* p. 75-6).

Ayer sees a common pattern in the sceptical argument in each case. Firstly, the sceptic argues that our knowledge of the conclusion depends solely on certain premises (e.g. we get knowledge of what is going on in other people's minds solely by observing their bodily actions). Secondly, it is argued that the relation between premises and conclusion is never deductive (anger-behaviour can never *entail* anger). Thirdly, it is argued that the inferences cannot be inductive either. For although an inductive argument can give us knowledge of objects we have never observed, it cannot give us knowledge of objects which it is logically impossible to observe. Yet physical objects, the past, other minds and scientific entities are unobservable in principle. The fourth step is to argue that inferences that cannot be justified deductively or inductively cannot be justified at all.

Different ways of trying to meet this line of argument mark off different philosophical theories. There are four main methods of rebuttal, each of which denies a different step in the sceptic's argument. The Naïve Realist denies the first step, he says that we do have a direct knowledge of the things in question. We directly perceive physical objects, are directly acquainted with the past, with other minds, etc. The Reductionist denies the second step, saying that there is a *deductive* connection between the evidence and the conclusion (e.g. behaviour statements and statements about minds). The scientific approach denies the third step, and argues that we have *inductive* knowledge of the matters in question. The method of Descriptive Analysis denies the

fourth step. It simply sets out the method of inference that we do use, admits that it is not deductive or inductive, but says it is none the worse for that. Ayer points out that the method used to solve one problem need not be the correct method of solving another. The present reviewer, for instance, realized that he was a Naïve Realist about the perception of physical objects and remembered past happenings, inclined towards Reductionism about other minds (and his own), and wavering between Reductionism and the Scientific Approach about scientific entities.

The last three chapters of the book show us Ayer actually at work on three of the problems, the perception of physical objects, memory-knowledge, and our knowledge of other minds. In this review I shall confine myself to what interests me most, his discussion of perception. In Chapter III he battles gamely to show that what is immediately given in perception is not physical objects, but simply sense-impressions i.e. to refute the Naïve Realist's rejection of the first step in the sceptic's argument. Ayer begins by discussing the main argument against Naïve Realism, the "Argument from Illusion". (It might be called, more accurately although more tediously, the "Argument from the incompatible variation of sensible appearances".) This argument may appeal either to variations in the appearance of real objects, or to total hallucinations. Ayer thinks the latter is the more important case. Surely there is *something* seen by the person who is hallucinated, even if what is seen is not a public object? At any rate, from the side of the observer at the time, such experiences are indistinguishable from veridical perceptions. But, as Ayer sees, the situation can also be described by saying that the person *thought* he saw something, although there was nothing to be seen there at all. (Ayer does not mention the fact, but along these lines we can even account for the parallelism between having an hallucination and actually seeing the corresponding physical object, a parallelism on which so many opponents of Naïve Realism have insisted. In the case where I am hallucinated I think I am seeing something, but I am wrong. In the case where I really do see something I think I am seeing something, *and am right*. Subjectively, then, the two cases must be identical, for the *only* difference between them is that in the second case my judgement really is right. Naturally, therefore, there can be no difference in my "experience" in each case.)

At this point Ayer considers whether we can bolster the argument from illusion by the argument from causation. Surely the fact that our perception of an object only occurs after all



sorts of complicated physical and physiological antecedents shows that what is immediately perceived cannot be the object itself, but is, perhaps, a sense-datum? Ayer does not think the argument is conclusive, but he does think that the Naïve Realist will have to concede that "the physical objects which we do perceive may owe some of their properties in part to the conditions which attend our perception of them" (p. 104, *Pel.* p. 95). Now it is true that this sometimes happens. I usually tell how hot or cold a thing is by touching it; but in some circumstances the actual touching might cool or warm the object touched. But it is hard to see why talk of light-waves and the stimulation of nerve-endings should force even *this* admission from the Realist. Can we not regard these occurrences as the mere conditions under which veridical perception occurs, conditions which enable us to perceive the object as it is without in any way altering it? The Naïve Realist will regard the physical and physiological antecedents of perception as impresarios who make no contribution to the performance of the actors (the nature of the physical objects), but which simply set up conditions where that performance can be *revealed*. The impresarios may falter, and then, but only then, the performance may seem to be other than it is. But normally, for all this argument shows, what we perceive may be quite unaffected by the conditions under which it is perceived. (Strangely enough, Ayer appears to admit this later (p. 128-9, *Pel.* p. 116-7).

It is true that there is one line of argument drawn from scientific discoveries that *does* seem to create a serious difficulty for Naïve Realism; but it is not the argument from the causation of perceptions. The difficulty is that the picture of the world presented to us by the physicist seems quite different from the way the world appears to perception (cf. Ayer p. 129, *Pel.* p. 117). I am not sure how the Naïve Realist should deal with this problem; in principle there seem to be three different lines of reply. Firstly, one might try to give a reductionist account of scientific entities, exhibiting talk about such things as electrons as being simply a way of talking about the ordinary things that we know. Secondly, one might give an account of these entities as being real objects over and above the things we perceive, discovered by scientific inference, and existing alongside ordinary objects. Thirdly, if these approaches did not do the whole job the Realist might have to admit that scientific discoveries had shown that perceptual illusion was a much more widespread phenomenon than we think it is when sunk in common sense. Perhaps there is something systematically misleading about most of my

perceptions. (He would go on to give an account of being under such illusions in Realist terms, i.e. as cases where we think that we perceive that objects have certain characters, but are in fact wrong.) And since for a Realist a thing's qualities are in no way constituted by the way things seem to people, there would be no *contradiction* in saying e.g. that this looks blue to everybody but is not really blue. At the same time, of course, it would need a great deal of evidence to persuade us of such a curious conclusion.

At any rate, Ayer thinks, although the Argument from Illusion and the causal argument may take some of the naïveté out of Naïve Realism, they are not conclusive against it. In Sections (iii) and (iv) he tries to "introduce the terminology of sense-data" in a somewhat different way. Firstly, he argues, in making statements of the sort "I see an X" I assert more than is warranted by my present experience; all that is really warranted is the statement "it now seems to me that I see an X". For wherever I can say the former the latter is implied, although the converse does not hold. We can then pass from "it now seems to me that I can see an X" to "I am now seeing a seeming-X". The "seeming-X" is an example of a sense-datum, and the "problem of perception" is the logical relation between sense-data and physical objects. At this point Ayer criticizes the Naïve Realist for simply ignoring this problem instead of trying to answer it. Now it is true that certain moderns have called themselves Naïve Realists and yet have simply contented themselves with reaffirming the validity of our ordinary speech-conventions, not realizing that this does not settle the question at issue between different philosophies of perception. (Their attitude is incredible in view of Berkeley's and others' painstaking re-affirmations of our ordinary linguistic distinction between e.g. appearance and reality. They have perhaps wrongly glossed Wittgenstein's dangerous remark that "ordinary language is all right".) But it seems to me that the Naïve Realist has, or at least ought to have, a view of the logical relations of "sense-datum statements" and physical object statements. He holds that no possible accumulation of sense-datum statements ever begins *logically to imply* the corresponding physical object statements, or vice versa. For since, according to Realism, physical objects exist independently of our perception or misperception of them there can be no logical links between sense-data and material objects.

Ayer discusses a number of objections to the introduction of the sense-datum terminology, two of which seem specially

important. He deals first with Ryle's view that perceptual verbs never signify the occurrence of an experience ("having a sense-datum"), because all they do is signal an achievement. He rejects Ryle's view on the grounds that while perceptual verbs are often used in the way that Ryle says that they always are, still, if I gaze at a paper I have something in sight, and "having something in sight" involves an experience, even if not one that gets much attention in ordinary discourse. It seems to me that a more profitable line of attack is to ask Ryle *what sort of thing seeing achieves*. Presumably, he would not want to say that what is achieved is an experience, for that would give Ayer his point. The only plausible answer seems to be that what is achieved is *knowledge*, knowledge about the objects before one's eyes. To say, "I see a cat" will have to be unpacked something like this: "By using my eyes I have come to know that there is a cat before me", where "using my eyes" means nothing more than turning these physical objects in the right direction. Ryle can then say that where I keep something in sight there is something going on, viz. my eyes remain pointing at the same thing. When I first look I come to know that there is a cat before me, at the next instant I come to know that the cat is still before me, and so on. But no further experience is involved. Being under sensible illusion, on this view, will be a matter of coming to have a false belief, or at least an inclination to believe falsely.

Now this view of perceiving as a species of coming-to-know has considerable plausibility. Perceiving certainly *involves* coming to know that something is the case, and being under sensible illusion involves coming to have a false belief, or at least an inclination to believe falsely. Still, Ayer might protest, granting this, perception involves much more. Consider the difference between coming to know that there is a cat in the next room by being told it, and actually *seeing* it. The point at issue here is substantially the same as that involved in a second criticism of the attempt to "introduce the language of sense-data" that Ayer discusses. Naïve Realists have argued that statements like "it appears red to me now" are not autobiographical statements recording certain "experiences" but simply express a tentative judgement, or an inclination to judge, that the object is red. Ayer admits that the form of words is often used in the way that Realists say it always is, but claims that the phrase may bear another sense:

"In the sense in which the word 'appear' is here being used, the way that things appear supplies both the cause of our tendency to judge that they really are whatever it may be and the ground



for the validity of these judgements. The judgements are not to be identified with their grounds, nor the tendency with their causes" (p. 112, Pel., p. 102).

This seems to be the crux of the matter. If Ayer is right here, then I think he is right to go on and argue as he does that the final step from "it seems to me that I perceive an X" to "I perceive a seeming-X" does not matter. Once admit that our ordinary judgements of perception are based on "experiences" which are identical whether the perception is veridical or not, and we have admitted that "our ordinary judgements of perception claim more than is strictly contained in the experiences on which they are based" (p. 125, Pel. p. 113). This means that there is a gap between evidence and conclusion, i.e. that the Naïve Realist is wrong in denying the first step in the sceptic's argument about perception.

It seems, then, that the Naïve Realist must deny that "perceptual experiences" are anything more than perceptual judgements, in some cases, perhaps, very tentative ones. And although it must be admitted that perception *feels* very different from other forms of judgement, the Realist can offer strong arguments for his view. Ayer speaks of the experiences as being the grounds of our perceptual judgement. Now the ground of a judgement is the evidence for it. But how can anything function as grounds or evidence for a judgement unless it is a judgement itself? It is true that we may speak of a happening or a state of affairs as constituting the evidence for a certain proposition. But it can only *function* as evidence if somebody makes a true judgement about it; this judgement can then be evidence for the truth of some other proposition. But the cat's being on the mat is not grounds or evidence for the judgement "the cat is on the mat". In the same way, if my perceptual experiences are not judgements they cannot be grounds or evidence for perceptual judgements. Contrariwise, if perceptual experiences *can* function in this way they must already be judgements.

Again, Ayer speaks of our perceptual experiences as the *causes* of our perceptual judgements. But the relation is certainly more intimate than that. When I have the perceptual experience described by saying "it looks red to me now" it is part of the essence or nature of this perceptual experience that I judge that there is a red physical object before me. Otherwise it would not *be* just this experience. Yet how can there be such a link unless the "experience" is itself a judgement that there is a red object before me? It is true that we can judge that a thing is not red although it looks red to me now, but even in this case if we went

simply on the way it looks now, abstracting e.g. from past experience, we would *have* to judge it was red. We cannot say "it looks red to me now (in the sense-datum sense), I have *no* independent reason to think it not red, but I do not believe it is red". But if there are these logical connections between "experience" and our judgements the experience must be judgement already. It is far from clear, therefore, that there really is a "gap" between our perceptual judgements and the "evidence" on which they are based—it seems, indeed, that they do not rest on evidence at all. Our right to be sure in the case of perceptual judgements seems to be earned not by arguing from "experiences", but by taking certain *precautions*, e.g. looking carefully, not deciding what the situation is after a mere fleeting glance, etc. And if it is asked how we know that certain procedures are good precautions against error, then in the end we can only say that we know they are.

Ayer, however, holds that the existence of such a "gap" has been demonstrated. He goes on to discuss the alternatives to Naïve Realism, viz. the Representative or Causal theory, on the one hand, and Phenomenalism on the other. He dismisses the former as being a scientific rather than a philosophical theory, but I shall omit this part of his discussion, because I do not properly understand it. Phenomenalism tries to solve the problem set by the sceptic by "reducing" physical objects to sense-experiences. Ayer considers certain well-known lines of objection to this programme.

In the first place, Phenomenalists are forced to render statements about unobserved physical objects as contrary-to-fact conditionals about the experiences of observers, and this has been objected to as turning unobserved objects into mere possibilities. Ayer offers the usual phenomenalist defence to this objection, pointing out that the phenomenalist does not deny that physical objects *actually* exist unobserved, but that he is offering an *account* of categorical statements about unobserved objects in terms of statements about sense-experiences that persons might have had. He admits that these phenomenalist accounts have a queer "feel" about them, but says that this is because Phenomenalism is associated with no picture of the world in the way that Realism and Representationalism are.

But, against Ayer, the trouble is not that the Phenomenalist gives us no picture, but that he gives us an unacceptable one. His picture of the world is that it consists of sense-data, the minds that have them, *and nothing else*. And unless we treat

unrealised possibilities in the Leibnizian way as being entities over and above actualities such a picture is inevitable, and seems to many philosophers a good reason to reject Phenomenalism. My unburned table could be called a permanent possibility of combustion, but it is not made of wood *plus* this permanent possibility. In the same way the Phenomenalist cannot treat the world as made up of minds and their sense-data, *plus* permanent possibilities of having more.

An even more serious difficulty for Phenomenalism is the problem of the observer. What account can the Phenomenalist give of the persons who "have" sense-experiences? Ayer leaves this problem aside and discusses it in his last chapter ("Myself and others", sec. ii). The difficulty is this. A sense-impression, we ordinarily think, cannot go around unattended, there must be persons who have the impression. Now what is a person? A phenomenalist cannot say a person is a physical object, an organism, because for him a physical object is a collection of sense-data. (And in any case Ayer is inclined to think that disembodied existence is logically possible, i.e. that we cannot identify a person with his body.) One possibility that may be suggested is that a person is a spiritual substance or soul, and that it is such substances that "have" sense-experiences. But Ayer thinks that this is really an unintelligible hypothesis, because no test would decide whether such substances existed or not. Ayer therefore falls back on the usual Phenomenalist view that the self is simply a *bundle* of experiences. For a person to have a sense-datum is for the sense-datum to be one member of a whole bundle of sense-data and other experiences which are the person. Such a conclusion is, indeed, inevitable if we take seriously the Empiricist contention that there can be no logical connections between distinct matters of fact, and if we agree that it is logically impossible for sense-data to exist outside a mind, i.e. if the notion of unsensed sensibilia is rejected as nonsensical. Ayer accepts the first principle and is inclined to accept the second. Once they are both accepted we see that sense-data cannot be distinct from the minds that have them. Berkeley's position, for instance, is really an inconsistent one. He says that the soul is entirely distinct from its ideas, yet ideas can only exist when perceived by a mind. But he is trying to have it both ways. If mind and its ideas are really distinct then they can exist apart, but if ideas cannot exist apart from a mind they are not distinct from it.

But the notion of the self as a bundle of experiences, while it seems to be entailed by Phenomenalism, faces desperate



difficulties. What is the relation that holds different members of the bundle together? Continuity of memory is necessary but not sufficient, for I do not remember every experience I have. "It needs to be backed by some other relation of which, perhaps, nothing more illuminating can be said than that it is the relation that holds between experiences when they are constituents of the same consciousness" (p. 226, Pel. p. 199). This is *ad hoc* postulation.

But the trouble is far more serious than just this. If the self really is a bundle of experiences held together by this special relation, is it not logically possible that each and any member of the bundle can exist apart? But this would seem to reintroduce the objectionable notion of an unhad or unsensed sense-datum. Ayer tries to get over the difficulty by saying that, while no particular sense-datum need be in the same bundle as another particular sense-datum, it is logically necessary that they be in some bundle. A sense-datum has as it were logical hooks which grapple it to *some* other sense-data even if there is no logical necessity about what sort of sense-data the others are. But this still involves logical connections between entities which in Ayer's view are distinct existences. In any case it is a very arbitrary business. One sense-datum cannot exist on its own. Can two? Or three? When does the bundle get big enough to permit of independent existence? The "problem of the observer" seems to be too much for Phenomenalism.

To return to chapter III, Ayer discusses there one more difficulty for Phenomenalism. If Phenomenalism is correct "there must be a deductive step from descriptions of physical reality to descriptions of possible, if not actual, appearances"; conversely, there must be "a deductive step from descriptions of actual, or at any rate possible, appearances to descriptions of physical reality" (p. 138-9, Pel. p. 124-5). But in fact, Ayer points out, these conditions cannot be satisfied. My sense-experiences, however extensive, never *force* on me just one description of physical reality, for it is always logically possible to explain the course of my sense-history as being due to some complex and extensive hallucination, even though the hypothesis we would have to use in the explanation might be quite fantastic. In the same way, there is no physical object so obtrusive that under certain conditions it must be observed, i.e. it must give rise to certain sense-data. It is always possible that object and observer were present, but that for some reason the observer failed to get the requisite sense-data. Ayer thinks that Phenomenalism cannot answer this objection.

But, he goes on to say: "The failure of Phenomenalism does not mean, however, that there is no logical connection between the way physical objects appear to us and the way they really are." He goes on to advocate a view which is really a sophisticated version of Phenomenalism. No set of sense-datum statements logically implies a physical object statement, and no physical object statement logically implies any sense-datum statements. But the two sorts of statement are logically linked. If we say that despite overwhelming sensory evidence a certain physical object statement is not true, we are logically committed to saying that there is an "explanation" for this, that other sense-data *must* be obtainable verifying the contradictory statement and showing how the deception was possible. These new sense-data do not themselves entail the truth of the new explanation, but if they are misleading there must be a still further "explanation" of the situation among the obtainable sense-data, and so on. In the same way, if a physical object is never observed there must be sense-data obtainable which would explain this failure to observe it. Hence, in effect, Ayer preserves Phenomenalism while evading the necessity of reducing physical object statements to sense-datum statements.

I think Ayer has succeeded here, but of course this reformulation of Phenomenalism would still have to meet the difficulty involved in treating unobserved objects as mere possibilities of having sense-data, and also the difficulty of giving an account of the observer, and I think he fails to meet these difficulties.

Here, then, perhaps, the Demon gives Ayer a fall. But the fact remains that in the first two chapters Ayer has given us a really thorough account of the way that an Empiricist will have to deal with scepticism. Even if the task is not completed, it has been well begun.

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## REVIEWS

INTRODUCTION TO LOGIC. By Patrick Suppes. (University Series in Undergraduate Mathematics.) Princeton: Van Nostrand, 1957, 312 p. \$5.50.

A MODERN INTRODUCTION TO LOGIC. By John W. Blyth. Boston: Houghton Mifflin Co., 1957. 426 p. \$5.50.

The appearance of this Suppes book is something of an event. It is one of a group of logic books which could well come to form an integral part of the professional reading of scientific men, in the way that logic books were an indispensable (and *used*) part of the reading of any learned man of the Middle Ages. (Another such book, somewhat smaller, but actually drawn on by Suppes here and there, is Tarski's earlier one with the same title.)

Not that Suppes's book (or Tarski's) gives us anything the least bit like the "inductive logic", "logic of discovery" or "scientific method" that so many Renaissance publicists thought would supersede the logic of the schools. That, we surely know by now, was mostly hot air; and what we have here is as formal as scholasticism ever was, and with a pattern that is now becoming a familiar one—truth-functional logic to start with, then predicates and quantifiers and the theory of identity, and then (after a metalogical interlude on use and mention and the theory of definition) a second major division of the book devoted to set theory, including the logic of relations and functions, i.e. of sets of ordered  $n$ -tuples. What is distinctive is the orientation of the whole towards a type of enterprise in which Suppes used to collaborate with the late J. C. C. McKinsey (it was in this connexion that one first heard of him), namely the embedding into logic and set-theory of any scientific discipline with enough rigour about it to permit of such treatment, e.g. classical and relativistic particle mechanics, the calculus of probabilities, the theory of measurement, and the theory of rational preference. (McKinsey, one might note as a matter of history, had in turn been earlier a collaborator of Tarski's, so we have here a body of logical literature with a certain connexion and coherence. McKinsey and Suppes were responsible for the review in the *Journal of Symbolic Logic* for March 1954, pp. 52-55, in which it was observed that "it should not be forgotten that about nineteen hundred years elapsed



between the time when Pilate asked his famous question 'What is truth?', and Tarski made a satisfactory reply".)

This formalising of exact sciences (or of sciences moving towards exactitude) is suggested in a number of instructive long examples scattered through Suppes's book, and then explicitly and systematically discussed in a final chapter. In this chapter much attention is paid to a certain device which might be illustrated as follows:—The relation of "being rated (by some specific person) at least as high as", call it  $Q$ , is clearly transitive, and it is possible to isolate a set of objects  $A$  such that with any pair  $x, y$  of these objects, either  $xQy$  or  $yQx$ . We might therefore formalise a fragment of the theory of rational preference by using variables  $x, y$ , etc. for objects of choice, and subjoining to any postulates we might have for pure logic the two axioms "if  $xQy$  and  $yQz$  then  $xQz$ " and "Either  $xQy$  or  $yQz$ ". (Given these, and the definition of " $x$  is preferred to  $y$ " as "Not  $yQx$ " and of " $x$  and  $y$  are regarded indifferently" as " $xQy$  and  $yQx$ ", it is surprising how much of the theory of preference is then easily deducible.) But, instead of thus introducing new symbols with new laws involving them, we might define *within* set-theory the notion of being a "preference pattern", by calling any set-*cum*-relation a preference pattern if the relation is transitive and either it or its converse holds between any two members of the set. In the same way, much more complicated special postulate-sets for the special notions of this and other disciplines which we might think of subjoining to logic and set-theory, can be replaced by definitions in purely set-theoretical terms of the relevant "patterns" or "systems". What this odd dodge has principally to recommend it is that it makes it easier to define the notion of a "model" for a theory and to prove various truths about such models. However one does it, it is certainly important to have the structures of, say, the theory of measurement and the theory of preference so clearly presented (and in a common symbolism) that one can see at a glance their resemblances and differences and the identity or difference of the strictly logical problems which each discipline poses. (Both the disciplines just mentioned, for example, have the problem of defining an equality or indifference which is transitive in terms of a perceived equality or indifference which is not.)

The only quarrel I have with this excellent book is this: In chapter 6, on "Use and Mention", p. 63, Suppes takes the line that any constant expression which may be substituted for a variable "must name some entity"; so that, for example, we

ought not to use variables like the  $p$  in "If  $p$  then  $p$ ", replaceable by sentences like "Grass is green," unless we believe that there are objects ("propositions", say) of which sentences are names—if we do not believe this we should stick to forms like " $P$  implies  $P$ ", where  $P$  is a variable replaceable not by a sentence but by the name of a sentence. He calls this (i.e. all variables being name-variables) "the standard viewpoint of formal logic"; which is a bit of swashbuckling, though Quine has given the view in question a pretty wide currency in America. Suppes goes on to describe the same view as "fundamental to the development of a sound, smooth-running logic of inference", but gives no reason for saying this beyond a weird argument on the following page, to the effect that, while "Every sentence  $P$  is either true or false" would be instantiated by "'Geronimo is dead' is either true or false", "Every proposition  $p$  is either true or false" would be instantiated by "Geronimo is dead is either true or false", which does not make sense. But either sentences name entities or they do not; if they do, "Geronimo is dead is either true or false" does make sense; while if they do not, there is no point in talking about "every proposition  $p$  . . ." For my own part, I should say that "Geronimo is dead is either true or false" does not make sense, but "It is either true or false that Geronimo is dead" does make sense, and instantiates, not any stuff about "propositions", or about sentences either, but "For any  $p$ , it is either true or false that  $p$ ".

John W. Blyth's *A Modern Introduction to Logic* is an altogether more lightweight book. It is for students who need to be told, for example, that "once a convention regarding the meaning of a word has been established, the question whether that word has zero or non-zero denotative value is no longer a matter of convention: it is a question of the facts of the case" (p. 59). In Suppes, the chapter on "Theory of Definition" deals with much trickier problems—rules for framing definitions so that they will not give rise to contradictions or surreptitiously alter the content of a theory, the correct formulation and use of conditional definitions like "If  $y$  is not equal to 0 then  $x/y = z$  if and only if  $x = yz$ ", proofs of independence of primitive symbols, and so on.

But I do not make this contrast entirely in Blyth's disparagement. We do need another type of logic book than Suppes's and that not only for the dumb clucks; also, e.g. for those whose culture is of a literary-historical type, who need logic as much as scientists and mathematicians do but not entirely the same bits of it and not in entirely the

same way. There ought to be some modern counterpart of the Port-Royal Logic, of Isaac Watts's logic, or best of all of Whately's *Elements*; or, to go back a bit further, we need not only the twentieth-century variant of the *Posterior Analytics* that a Suppes or a Tarski can give us, but something more like the *Topics* and *Sophistical Refutations*. That is where Blyth's book belongs; and in that context it has its points too, but also at least one shocking mistake which makes one wish he had read more or consulted more or better colleagues.

Like Aristotle in the *Topics*, Blyth makes much use of dichotomy, and evolves criteria of reasoning out of continual dichotomous classifications. His *Modern Introduction* is studded all over with "matrices", variations of the familiar truth-tables; there is even a certain matrix-frame (quite a pleasing one) inscribed on the book's cover and reappearing as an ornament beside all chapter headings. And while he maybe overdoes it a bit, I think he's got something here—this device is one that could and should be put to wider logical uses than it usually is. In particular he introduces class-logic and syllogistic logic by listing 16 different possibilities one has with a pair of terms A and B—representing non-B as b and non-A similarly, we might have AB's, Ab's, aB's and ab's all existing; or AB's, Ab's and aB's existing but ab's not; and so on. Different propositional forms are defined as disjunctions of these, e.g. "No A's are B's" as the disjunction of the eight cases in which there are no AB's; and a whole system of logic is built upon the use of these tables.

What Blyth has here is not quite the standard modern class-logic, since this assumes a non-empty universe and one of Blyth's 16 possibilities is that in which there are no AB's, Ab's, aB's or ab's; nor is it Aristotelian logic either, since that excludes all of Blyth's 16 cases except the seven in which neither A, B, a nor b is empty. It is going much too far, however, to say as Blyth does in p. 224 n. 1 that "a non-existential logic of classes has not been presented heretofore". There is exactly that—i.e. a logic which allows for the possibility of the universe being empty—in, for example, the Quantified Logic of Properties sketched by von Wright in *On the Idea of Logical Truth* (I), Helsingors 1948; and Keynes took the same "non-existential" line in ed. 2, p. 145 n. 1, though he abandoned it in ed. 3, p. 191 n. 4. But in what Blyth does next, developing the current class-logic and Aristotelian logic from this "non-existential" sort by striking out the appropriate lines from his tables, there is indeed something which I do not think has



been done so explicitly in English before (though the heart of it is in de Morgan's *Formal Logic* Ch. 4 and 5, and it is very thoroughly done in German in Menne's *Logik und Existenz*, 1954). This makes it all the more regrettable that just at this point Blyth makes his big slip. He says that both modern class-logic and Aristotelian logic exclude from consideration not only the case in which all four subdivisions of the universe are empty but also the case on which none of them are (thus cutting down the cases considered by the modern from 15 to 14 and by the Aristotelian from 7 to 6). His reason for saying this is that both moderns and Aristotelians assume that there are true universal propositions, and the truth of a universal proposition requires the non-existence of some combination of terms. Yes, but not necessarily of some combination of a given A and B. No modern and no Aristotelian logician would argue (as Blyth depicts them doing on p. 227): "There are white men, there are white non-men, there are non-white non-men, therefore there are no non-white men"—not even when you re-word the argument as "Some men are white, some things that aren't men are white, some things that aren't men aren't white either, therefore all men are white".

Some of the examples in both these books provide instructive data for anthropologists studying the American culture-pattern. Blyth (p. 349) confronts us with this dilemma: "You cannot marry me unless you get a divorce from Peggy. And I won't marry you unless you can support me. Furthermore, if you get a divorce from Peggy, you will have to pay a large alimony. But you cannot pay a large alimony and support me. Hence you cannot marry me." Suppes has copious exercises (pp. 51-2) in symbolising such statements as "Betty is pretty but not dignified", "There are both seniors and juniors who date Betty", "Some freshmen date only seniors who like Greek", and for the more advanced (pp. 56-7), "Some freshmen who like both Greek and mathematics date neither Betty nor Elizabeth" and "If seniors date only juniors then some seniors date no one". We also learn from Blyth (p. 31) that if someone says to you "I saw your girl at the dance last night with your friend George" when in fact he did not, he has committed an "informative functional fallacy". I'll say he has.

A. N. PRIOR

SOVEREIGN REASON, AND OTHER STUDIES IN THE PHILOSOPHY OF SCIENCE. By Ernest Nagel. Glencoe, Illinois: The Free Press, 1954. 308 p. \$5.00.

LOGIC WITHOUT METAPHYSICS, AND OTHER STUDIES IN THE PHILOSOPHY OF SCIENCE. By Ernest Nagel. Glencoe, Illinois: The Free Press, 1956. 427 p. \$6.00.

The earlier book contains sixteen essays, the later thirty essays and reviews. Apart from the introductions, the entire contents of both books have appeared before, viz. between 1930 and 1954, in publications ranging from *The New Republic* and *The Saturday Review* to the specialist journals and various symposium volumes.

*Sovereign Reason* (*S.R.* from now on) consists mainly of critical expositions of some philosophical theories bearing on science. Dewey, Peirce, and Russell account among them for half the essays. Nagel's own philosophy is more explicitly presented in *Logic Without Metaphysics* (*L.W.M.* from now on). He calls it "contextualistic naturalism": "naturalism" because it is empiricist and vaguely materialist; "contextualistic" because Nagel thinks that many philosophical errors arise from the neglect of the contexts in which concepts are employed and statements made.

The exposition is always good. The picture of "Analytic Philosophy in Europe" (1936, *L.W.M.* pp. 191-246) is historically very interesting. In some respects the Deweyan emphasis on use and context has certainly anticipated the lessons of later British philosophy about meaning. Finally, it is as pleasant as it is uncommon nowadays to be told by a philosopher what he thinks there is, as Nagel tells us in the papers on naturalism in *L.W.M.* The only pieces of fresh, first-hand philosophising, however, seem to me to be the discussion of Reichenbach's philosophy of probability (*S.R.* pp. 225-248) and part of the discussion of the laws of thought (*L.W.M.* pp. 60-64). A great proportion of each book is given over to the reiteration of criticisms, mostly stale and superficial, of a small number of philosophical doctrines: e.g. of the denial of the objectivity of secondary qualities; of doctrines which make a problem of our knowledge of the external world; of epistemologies which claim to start from simple data. There is nothing in all this to merit the attention of philosophers. But taking these books in an overall way, there are two topics on which Nagel is ambiguous and on which his ambiguity is philosophically significant.

The first is the status of science itself. Nagel often defends science, against its enemies and false friends, in the name of "reason" and "sanity". Indeed this seems to be his intellectual *raison d'être*. Yet in his slightly less polemical moments he gives me, at any rate, the impression that the distinctive methods of science are "scientific" only in the sense that they are the distinctive methods of the persons properly called scientists. Now there is a very profound problem here. If rationality can only be defined by reference to the actual history of science then it will be truistic, and polemically useless, to say that the methods of science are the voice of reason; and if any other property, e.g., "shy musical pipe-smoker" tends to be common and peculiar to the persons rightly called scientists, then this property will have as good a claim to be part of rationality as the predicate "argues according to the Method of Difference". Nagel is always saying that the methods of science (not merely the content) are historical phenomena, subject to correction, and warranted by their predictive success; so perhaps he would say that the propensity to argue according to the Method of Difference is just something historically true especially of scientists, like the propensity to shyness, musicalness and pipe-smoking (*supposing* that to be true especially of them). At any rate, he is not provoked to any reconsideration of the grounds of his partisanship of science by writers who point out that without *a priori* principles of evidence even predictive success "warrants" nothing, and who therefore try to show that there is a perfectly good *strong* sense in which the kind of thing scientists do is scientific. See, for example, his review of Williams's *The Ground of Induction* (*L.W.M.* pp. 335-346). This ambiguity is not peculiar, of course, to Nagel's writings; but the readiness with which, according as it suits him, he espouses either side is unphilosophical to a degree uncommon even in the philosophy of science.

The second big source of ambiguity throughout these books is the status of ethics. The problem of reconciling naturalism with the point of view of morality is a more obvious and familiar one than that mentioned in the paragraph above. Indeed Nagel's hedgings and hectorings here are depressingly predictable. He will not say, of course, that some ethical convictions or social policies are true or are scientific and some not. On the other hand, he scorns no rhetorical device that may serve somehow to align certain ethical convictions or social policies with the scientific temper and contrary ones with metaphysics, theology, fanaticism, etc., etc. The following passage is representative. Contextualistic naturalism "does not conceive the



primary moral problem to be that of discovering or actually instituting some fixed set of ethical norms valid everywhere and for all time. For basic moral problems are plural in number and specific in character, and are concerned with the adjustment, in the light of causes and consequences, of competing impulses occurring in specific environmental contexts. There can therefore be no general or final solution to the moral predicaments of mankind; the moral problem is the perennial one of finding ways and means for eliminating needless suffering and for organising in a reasonable manner the energies of men" (*S.R.* p. 56). Such a perfect fusillade of illogicalities is not typical of these books, nor is Nagel's style always so inflated. But it should be obvious that there is no danger of mistaking what Nagel writes about ethics for moral philosophy.

A philosopher in earnest with "contextualistic naturalism" might reasonably be expected to engage in investigation of concepts central to such a world-view, such as space, time, matter, and individuals. Not so Nagel. Instead he strikes an attitude of sturdy scepticism about ghosts! (*L.W.M.* p. 7). Indeed the whole purpose of most of these pieces is polemical and edificatory, in the manner of a generation of American popular philosophers one had hoped was extinct. That is why the result is philosophically negligible even when Nagel handles topics more in his chosen line, like final causes (*L.W.M.* p. 421) or potentiality (*L.W.M.* p. 158) or self-evidence (*L.W.M.* p. 165 and ff., and *S.R.* p. 292). He does not produce discussions at all; as a propagandist for the Primitive Dogmatic Empiricist People's Party, his reflexes just react automatically to any bit of the phraseology of intellectual "reaction", and lo! another doctrinal exercise is in the press. (Final causes, for example, are "those barren vestal virgins of medieval science".) From this homiletic point of view, of course, there is nothing wrong with superficiality and repetition; nor is any writer, even an Aldous Huxley, a Lecomte du Noüy, or a Maurice Cornforth, too inconsiderable to furnish a text for yet another lay sermon in praise of science. "Philosophy of science" indeed! "Studies" indeed! A better sub-title would have been "Reassurance for *Partisan Reviewers*" (*S.R.* is dedicated to Sydney Hook), reassurance that secularism is still all right. So it is, and so may it ever be; but these books are no advertisement for it.

Misprints are common in both books (especially so in the later one) and some are serious, e.g. in the last line of the footnote, p. 227 *L.W.M.*, "P1" should be "P2".

D. C. STOVE

## BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

BAHM, A. J. *Philosophy of the Buddha*. London, Rider, 1958. 175 p. 12s. 6d. (U.K.); 20s. 9d. (Australian).

BANERJEE, Nikunja Vihari. *Concerning human understanding; essays on the common-sense background of philosophy*. London, Allen and Unwin, 1958. 333 p. 30s. (U.K.).

The first two Parts ("Prelogomena to a theory of knowledge" and "Our knowledge of the external world") lead to the conclusion that understanding the world is the business not of philosophy but of science, which is common sense systematised. Part three ("What, then, is philosophy?") argues that philosophy is concerned with "the self as subject" and with moral and æsthetic values. Part four ("Religion within the bounds of practical reason") advocates humanism—"a religion of man" as distinct from "the old religion of God"—"as revealed in the light of the wisdom of Buddha and most prominently symbolized by the life of Jesus".

BECK, William S. *Modern science and the nature of life*. London, Macmillan, 1958. xix, 302 p. 25s. (Australian).

An account of biology for the general reader, with some attention to philosophical questions, such as the nature of scientific explanation.

BEGG, John Campbell. *Essays on thoughts and worlds*. Dunedin, N.Z., Coulls Somerville Wilkie Ltd., 1958. 187 p. 15s.

A collection of papers and articles written between 1899 and 1952. Some have been published in philosophical journals, including this one. The author is chiefly interested in astronomy, time and related topics.

BERKELEY, George. *The works of George Berkeley, Bishop of Cloyne*, edited by A. A. Luce and T. E. Jessop: Volume nine. London, Nelson, 1957. vi, 191 p. 30s. (U.K.).

The final volume, containing notes to Berkeley's letters, addenda and corrigenda to the earlier volumes, a general index to all nine volumes, a chronology of Berkeley's life and a list of his writings.

BURNETT, Whit, ed. *This is my philosophy: twenty of the world's outstanding thinkers reveal the deepest meanings they have found in life*. London, Allen and Unwin, 1958. xix, 378 p. 25s. (U.K.).

The twenty are: Bertrand Russell, Lewis Mumford, J. B. S. Haldane, Aldous Huxley, G. M. Trevelyan, J. R. Oppenheimer, Frank Lloyd Wright, P. A. Sorokin, Jaspers, Heisenberg, Jung, Sartre, Schweitzer, Ignazio Silone, Maritain, Niebuhr, W. E. Hocking, Gabriel Marcel, de Madariaga, and Radhakrishnan.

CALVIN, John. *On the Christian faith; selections from the Institutes, Commentaries, and Tracts*, edited, with an introduction, by John T. McNeill. New York, Liberal Arts Press, 1957. xxxiii, 219 p. Paper covers. 90c.

CHANNING, William Ellery. Unitarian Christianity and other essays, edited, with an introduction, by Irving Bartlett. New York, Liberal Arts Press, 1957. xxxii, 121 p. Paper covers. 80c.

COMPTON, Charles H., comp. William James, philosopher and man: quotations and references in 652 books. New York, Scarecrow Press, 1957. 229 p. \$4.50.

"Part one consists of quotations from 264 books by 146 authors. Part two consists of references to 652 books by 344 authors. The references include those books which are in Part one."—Introduction.

FAKHRY, Majid. Islamic occasionalism; and its critique by Averroës and Aquinas. London, Allen and Unwin, 1958. 220 p. 21s. (U.K.).

"Long before [Malebranche] the Moslem theologians of the ninth and tenth centuries had developed an occasionalist metaphysic of atoms and accidents, which was inspired by the same theological motives which had inspired Malebranche, viz. the vindication of the omnipotence of God and the powerlessness of man . . .

"Dr. Fakhry contends that a number of distinctively Islamic concepts such as fatalism, utter resignation to God, the surrender of personal endeavour . . . cannot be fully understood save in the perspective of the occasionalist world view of Islam, expounded and discussed in this work."—Blurb.

GREENE, Theodore Meyer. Moral, æsthetic and religious insight. New Brunswick, Rutgers University Press, 1957. 141 p. \$2.75.

Argues that Kant "was so successful that we would to-day be well advised to extend his method further than he himself was able to do by applying his basic epistemological concepts and criteria not only to moral values but to the realms of beauty and holiness"; and attempts, in outline, to do this.

HARTLAND-SWANN, John. An analysis of knowing. London, Allen and Unwin, 1958. 141 p. 15s. (U.K.).

Discusses the differences between "knowing Jones", "knowing French", "knowing how", "knowing what to do", etc. etc., and goes on to draw some general epistemological conclusions. Many of the chapters are based on articles previously published in various philosophical journals, including this one.

HENDEL, Charles W., ed. The philosophy of Kant and our modern world; four lectures delivered at Yale University commemorating the 150th anniversary of the death of Immanuel Kant. New York, Liberal Arts Press, 1957. vii, 132 p. \$2.75.

The lectures are by John E. Smith ("The Question of Man"), George E. Schrader ("The Philosophy of Existence") and Charles W. Hendel ("Freedom, Democracy and Peace"), all of the Philosophy Department at Yale, and René Wellek ("Æsthetic and Criticism"), Professor of Comparative Literature. They were intended to introduce Kant to the general student body.

HENLE, Paul, ed. Language, thought and culture. Ann Arbor, University of Michigan Press, 1958. vi, 273 p. \$4.95.

This book arose out of a series of symposia at Michigan in which a number of philosophers, psychologists, sociologists and language specialists from several American universities discussed such topics as the



way concepts are formed, artificial languages, the difference between "cognitive" and "non-cognitive" language, and so on. The authors are: Roger W. Brown, Irving M. Copi, Don E. Dulaney, William K. Frankena, Paul Henle, and Charles L. Stevenson.

LECLERC, IVOR. Whitehead's metaphysics; an introductory exposition. London, Allen and Unwin, 1958. xiii, 234 p. 21s. (U.K.).

"It is the particular thesis of this book that in developing the system which he elaborated in such detail in *Process and Reality*, Whitehead's problems were specifically metaphysical and not those which characterized his earlier investigations in the philosophy of natural science. I have tried to make clear that Whitehead's basic problems belong to the great tradition of philosophical inquiry, first opened up by the Greeks. I have set out to explain how and why Whitehead posed the questions in a way different from that of antecedent thought, and that this is the root of the originality and novelty of his philosophy."—Preface.

LONGACRE, Robert E. Proto-Mixtecan. Publication 5 of the Indiana University Research Center in Anthropology Folklore and Linguistics; also Part III of the International Journal of American Linguistics, v. 23, no. 4, Oct. 1957. Bloomington, Indiana University, 1957. viii, 195 p. Paper covers. \$3.50.

MARTIN, R. M. Truth and denotation; a study in semantical theory. London, Routledge and Kegan Paul, 1958. xii, 303 p. 42s. (U.K.).

"In short, this book contains (1) a rather full exposition of some semantical theories closely akin to those first studied by Tarski and Carnap; (2) the formulation of a new minimal semantic theory of a non-translational kind . . . ; (3) the formulation of two kinds of semantical theories, one translational and one non-translational, in which inscriptions or sign events are taken as values for expressionable variables; and (4) an argument concerning the importance for philosophical analysis and the methodology of science of denumerable formalized language-systems of first order."—p. 30.

MAYO, Bernard. Ethics and the moral life. London, Macmillan, 1958. viii, 238 p. 34s. 9d. (Australian).

"Though written in a contemporary idiom and relying on modern analytical techniques, this book departs from other recent books on ethics in acknowledging the task of relating moral theory not only to actual moral discourse, but also to metaphysical theories about the make-up of man as a moral being."—Blurb.

MEHLBERG, Henryk. The reach of science. Toronto, University of Toronto Press, 1958. xii, 356 p. \$5.50.

"This monograph . . . centres round a single question . . . : What theoretical and practical problems could possibly be solved by applying scientific method? To what problems is this method inapplicable? The answer asserts the inherent universality of science: if a problem is solvable at all it can be solved, in principle, by applying scientific method."—Preface.

MILL, John Stuart. Considerations on representative government, edited, with an introduction, by Currin V. Shields. New York, Liberal Arts Press, 1958. xlv, 275 p. Paper covers. 90c.

MOLESWORTH, Vol. Landmarks in Logic. Sydney, Law Book Co. of Australasia, 1958. xvi, 140 p. 25s. (Australian).

A brief, partly historical survey, written for "newcomers to philosophy", of what the author says he takes to be the central issues in the development of logical theory. However, excepting those of Socrates and Aristotle, the views discussed (those of Descartes, Locke, Kant, Mill, Russell, etc.) are treated as if they were landmarks in the decline rather than the development of the subject.

ROMANELL, Patrick. Toward a critical naturalism; reflections on contemporary American philosophy. New York, Macmillan Co., 1958. xv, 88 p. \$3.25.

Six brief, and very slight, essays, three on metaphysics and three on ethics. The author, who is Professor of Medical Philosophy and Ethics at the University of Texas (Medical Branch), wants a metaphysic that will combine materialism, idealism, dualism and phenomenism, and an ethic that will "be a reconciliation of Immanuel Kant and John Stuart Mill". He does not tell us how these *mariages de convenance* are to be arranged.

ROSSI-LANDI, Ferruccio, ed. Il pensiero americano contemporaneo: scienze sociali. Milan, Edizioni di Comunità, 1958. xii, 392 p. L 4,000.

ROSSI-LANDI, Ferruccio, ed. Il pensiero americano contemporaneo: filosofia, epistemologia, logica. Milan, Edizioni di Comunità, 1958. xii, 342 p. L 4,000.

Each volume contains eight or nine separate chapters, by different authors, on quite distinct topics. These include (in the philosophy volume): operationalism in physics, modal logic, ideal languages, and the methodology of history; and (in the social sciences volume): the concepts of "culture" and "personality", positivism in jurisprudence, the Kinsey report, and democracy. The authors are all Italian philosophers or sociologists.

ROTHSTEIN, Jerome. Communication, organization and science; with a foreword by C. A. Muses. Indian Hills, Colorado, Falcon's Wing Press, 1958. xcvi, 110 p. \$3.50.

The author, a physicist, excuses this book on the grounds that "life is short, the need for synthesis is great, and the vistas are exhilarating". His synthesis, well-written if naive and rather thinly spread, is in terms of "organisation"—to wit, negative entropy. Most of the ideas come (roughly speaking) from Norbert Wiener. The editor contributes a pompous and patronising 85-page foreword in which he takes the opportunity to horn in with a few ideas of his own on Diophantine equations, Bode's law and what he calls "chronotopology": the relevance of this is explained but not made clear. C.L.H.

SPINOZA, Baruch. On the improvement of the understanding; translated, with an introduction, by Joseph Katz. New York, Liberal Arts Press, 1958. xx, 40 p. Paper covers. 50c.

SULLIVAN, Celestine J., Jr. Critical and historical reflections on Spinoza's "Ethics". (University of California Publications in Philosophy, volume 32.) Berkeley, University of California Press, 1958. 45 p. Paper covers. \$1.

Argues that "all the many paradoxes in Spinoza's thought are but manifold expressions of a single paradox with a single cause: Spinoza



is at one and the same time both a materialist and an idealist, a naturalist and a supernaturalist. The cause of this ambiguity is simply the divided character of his mind . . . : a division of mind quite characteristic of his age and given poignant expression by men of letters like John Donne and Sir Thomas Browne." (pp. 2-3).

TESTA, Aldo. *La dialogica universale*. Bologna, Cappelli, 1957. 237 p. Paper covers. L 850.

TESTA, Aldo. *Somma dialogica*. Bologna, Cappelli, 1957. 141 p. Paper covers. L 500.

TESTA, Aldo. *La scuola del dialogo*. Bologna, Cappelli, 1958. 344 p. Paper covers. L 1,500.

TESTA, Aldo. *Il dialogo sociale*. Bologna, Cappelli, 1958. 205 p. Paper covers. L 800.

The first two of these volumes are concerned to expound "the dialectical structure of reality", while the third and fourth deal respectively with the implications for education and politics. The author, who teaches philosophy at the University of Bologna, is also the editor of a quarterly called *Il Dialogo*.

TOULMIN, S. E. *The uses of argument*. Cambridge University Press, 1958. 272 p. 22s. 6d. (U.K.).

Argues that regarding logic as a formal science has created unreal problems in philosophy (e.g. about induction) and has improperly brought discredit on the modes of reasoning peculiar to the various provinces of knowledge and practice. There are provocative views expressed on the concepts probability, analyticity, validity, etc. The most elaborate part of the book is a suggested new "layout" for syllogistic arguments with a singular premise, employing the distinctions between premises, inference "warrants", and the backing for these warrants.

ULLMANN, Stephen. *The principles of semantics*. (Glasgow University Publications LXXXIV.) 2nd ed. Glasgow, Jackson; Oxford, Blackwell, 1957. 346 p. 64s. 9d. (Australian).

First published in 1951. This edition has a supplementary chapter on "Recent Developments in Semantics", and an enlarged bibliography.

UNIVERSITY OF COLORADO STUDIES: Series in philosophy: No. 1, studies in ethical theory. Boulder, University of Colorado Press, 1958. 111 p. Paper covers. \$2.50.

Contents: "Ethics and human nature", by Bertram Morris; "'Human nature' and ethics", by Edward J. Machle; "Philosophical ethics and morality", by John O. Nelson; "On the justification of ought-statements", by Robert Rogers; "Ethics and ethical experience", by David Hawkins; "The need for sound type-theory in ethical inquiry", by Doris Webster Havice; "Human nature, science and philosophy", by William Sacksteder; "What is truth about man?", by Forrest Williams.

VALENTINI, Francesco. *La filosofia francese contemporanea*. Milan, Feltrinelli, 1958. 371 p. L 2,500.

Discusses the way in which, since the war, there has been a general tendency in France to give new twists to such systems of thought as existentialism, Marxism, Hegelianism, and even traditional Catholic Christianity. Among the authors discussed are Sartre, Maurice Merleau-Ponty, Gabriel Marcel, Emmanuel Mounier, and Eric Weil.